

SCIENCE

JULY 7, 1950



OPEN LETTER TO
THE UNITED NATIONS

NIELS BOHR

TECHNICAL PAPERS

COMMENTS AND COMMUNICATIONS

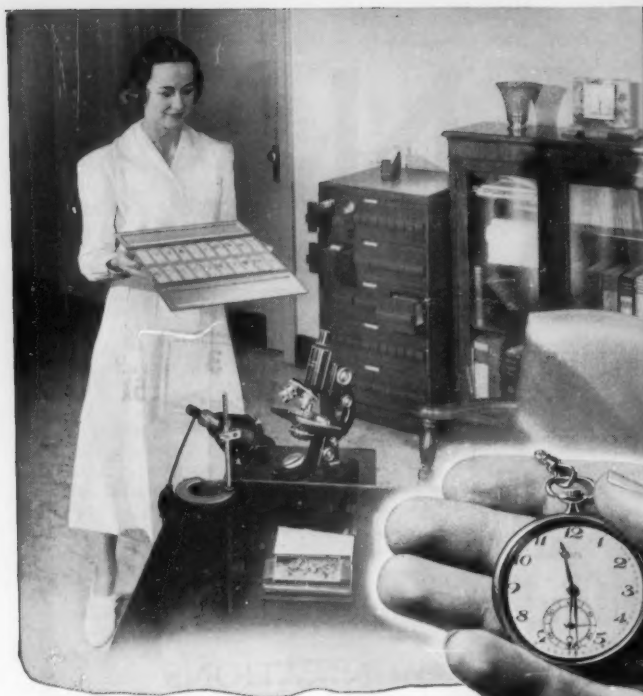
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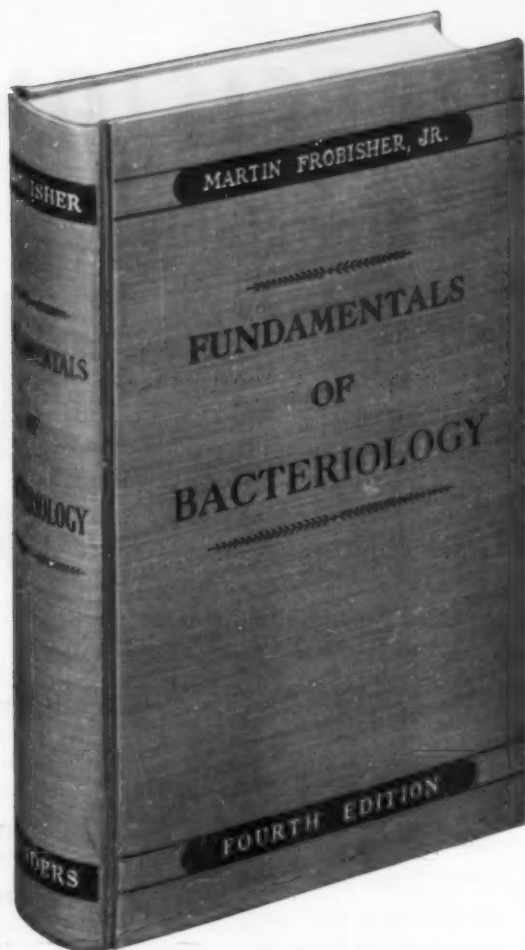
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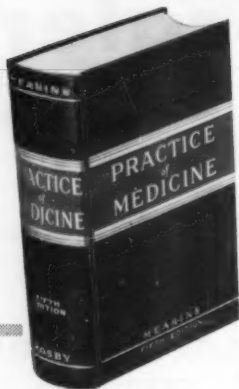
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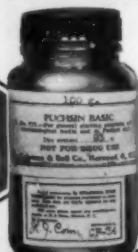
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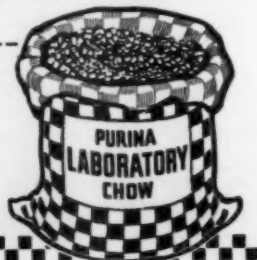
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Open Letter to the United Nations¹

Niels Bohr

Copenhagen, Denmark

I ADDRESS MYSELF to the organization founded for the purpose to further cooperation between nations on all problems of common concern, with some considerations regarding the adjustment of international relations required by modern development of science and technology. At the same time as this development holds out such great promise for the improvement of human welfare it has, in placing formidable means of destruction in the hands of man, presented our whole civilization with a most serious challenge.

My association with the American-British atomic energy project during the war gave me the opportunity of submitting to the governments concerned views regarding the hopes and the dangers which the accomplishment of the project might imply as to the mutual relations between nations. While possibilities still existed of immediate results of the negotiations within the United Nations on an arrangement of the use of atomic energy guaranteeing common security, I have been reluctant in taking part in the public debate on this question. In the present critical situation, however, I feel that an account of my views and experiences may perhaps contribute to renewed discussion about these matters so deeply influencing international relationship.

In presenting here views which impressed themselves at an early stage on a scientist who had the opportunity to follow developments at close hand I am acting entirely on my own responsibility and without consultation with the government of any country. The aim of the present account and considerations is to point to the unique opportunities for furthering international understanding and cooperation that have been created by the revolution of human resources brought about by the advance of science, and to stress that despite previous disappointments these opportunities still remain and that all hopes and all efforts must be centered on their realization.

For the modern rapid development of science and in particular for the adventurous exploration of the properties and structure of the atom, international cooperation of an unprecedented extension and intensity has been of decisive importance. The fruit-

fulness of the exchange of experiences and ideas between scientists from all parts of the world was a great source of encouragement to every participant and strengthened the hope that an ever closer contact between nations would enable them to work together on the progress of civilization in all its aspects.

Yet, no one confronted with the divergent cultural traditions and social organization of the various countries could fail to be deeply impressed by the difficulties in finding a common approach to many human problems. The growing tension preceding the second world war accentuated these difficulties and created many barriers to free intercourse between nations. Nevertheless, international scientific cooperation continued as a decisive factor in the development which, shortly before the war, raised the prospect of releasing atomic energy on a vast scale.

The fear of being left behind was a strong incentive in various countries to explore in secrecy the possibilities of using such energy sources for military purposes. The joint American-British project remained unknown to me until, after my escape from occupied Denmark in the autumn of 1943, I came to England at the invitation of the British government. At that time I was taken into confidence about the great enterprise which had already then reached an advanced stage.

Everyone associated with the atomic energy project was, of course, conscious of the serious problems which would confront humanity once the enterprise was accomplished. Quite apart from the role atomic weapons might come to play in the war, it was clear that permanent grave dangers to world security would ensue unless measures to prevent abuse of the new formidable means of destruction could be universally agreed upon and carried out.

As regards this crucial problem, it appeared to me that the very necessity of a concerted effort to forestall such ominous threats to civilization would offer quite unique opportunities to bridge international divergencies. Above all, early consultations between the nations allied in the war about the best ways jointly to obtain future security might contribute decisively to that atmosphere of mutual confidence which would be essential for cooperation on the many other matters of common concern.

¹ This letter was delivered to the Secretary General of the United Nations on June 12, 1950, and at the same time released for publication.

In the beginning of 1944, I was given the opportunity to bring such views to the attention of the American and British governments. It may be in the interest of international understanding to record some of the ideas which at that time were the object of serious deliberation. For this purpose, I may quote from a memorandum which I submitted to President Roosevelt as a basis for a long conversation which he granted me in August 1944. Besides a survey of the scientific background for the atomic energy project, which is now public knowledge, this memorandum, dated July 3, 1944, contained the following passages regarding the political consequences which the accomplishment of the project might imply:

It certainly surpasses the imagination of anyone to survey the consequences of the project in years to come, where in the long run the enormous energy sources which will be available may be expected to revolutionize industry and transport. The fact of immediate preponderance is, however, that a weapon of an unparalleled power is being created which will completely change all future conditions of warfare.

Quite apart from the question of how soon the weapon will be ready for use and what role it may play in the present war, this situation raises a number of problems which call for most urgent attention. Unless, indeed, some agreement about the control of the use of the new active materials can be obtained in due time, any temporary advantage, however great, may be outweighed by a perpetual menace to human security.

Ever since the possibilities of releasing atomic energy on a vast scale came in sight, much thought has naturally been given to the question of control, but the further the exploration of the scientific problems concerned is proceeding, the clearer it becomes that no kind of customary measures will suffice for this purpose and that especially the terrifying prospect of a future competition between nations about a weapon of such formidable character can only be avoided through a universal agreement in true confidence.

In this connection it is above all significant that the enterprise, immense as it is, has still proved far smaller than might have been anticipated and that the progress of the work has continually revealed new possibilities for facilitating the production of the active materials and of intensifying their effects.

The prevention of a competition prepared in secrecy will therefore demand such concessions regarding exchange of information and openness about industrial efforts including military preparations as would hardly be conceivable unless at the same time all partners were assured of a compensating guarantee of common security against dangers of unprecedented acuteness.

The establishment of effective control measures will of course involve intricate technical and administrative problems, but the main point of the argument is that the accomplishment of the project would not only seem to necessitate but should also, due to the urgency of mutual

confidence, facilitate a new approach to the problems of international relationship.

The present moment where almost all nations are entangled in a deadly struggle for freedom and humanity might at first sight seem most unsuited for any committing arrangement concerning the project. Not only have the aggressive powers still great military strength, although their original plans of world domination have been frustrated and it seems certain that they must ultimately surrender, but even when this happens, the nations united against aggression may face grave causes of disagreement due to conflicting attitudes towards social and economic problems.

By a closer consideration, however, it would appear that the potentialities of the project as a means of inspiring confidence just under these circumstances acquire most actual importance. Moreover the momentary situation would in various respects seem to afford quite unique possibilities which might be forfeited by a postponement awaiting the further development of the war situation and the final completion of the new weapon. . .

In view of these eventualities the present situation would seem to offer a most favourable opportunity for an early initiative from the side which by good fortune has achieved a lead in the efforts of mastering mighty forces of nature hitherto beyond human reach.

Without impeding the importance of the project for immediate military objectives, an initiative, aiming at forestalling a fateful competition about the formidable weapon, should serve to uproot any cause of distrust between the powers on whose harmonious collaboration the fate of coming generations will depend.

Indeed, it would appear that only when the question is taken up among the united nations of what concessions the various powers are prepared to make as their contribution to an adequate control arrangement, it will be possible for anyone of the partners to assure themselves of the sincerity of the intentions of the others.

Of course, the responsible statesmen alone can have the insight in the actual political possibilities. It would, however, seem most fortunate that the expectations for a future harmonious international co-operation which have found unanimous expression from all sides within the united nations, so remarkably correspond to the unique opportunities which, unknown to the public, have been created by the advancement of science.

Many reasons, indeed, would seem to justify the conviction that an approach with the object of establishing common security from ominous menaces without excluding any nation from participating in the promising industrial development which the accomplishment of the project entails will be welcomed, and be responded with a loyal co-operation on the enforcement of the necessary far reaching control measures.

Just in such respects helpful support may perhaps be afforded by the world-wide scientific collaboration which for years has embodied such bright promises for common human striving. On this background personal connections between scientists of different nations might even

offer means of establishing preliminary and non-committal contact.

It need hardly be added that any such remark or suggestion implies no underrating of the difficulty and delicacy of the steps to be taken by the statesmen in order to obtain an arrangement satisfactory to all concerned, but aim only at pointing to some aspects of the situation which might facilitate endeavours to turn the project to lasting benefit for the common cause.

The secrecy regarding the project which prevented public knowledge and open discussion of a matter so profoundly affecting international affairs added, of course, to the complexity of the task of the statesmen. With full appreciation of the extraordinary character of the decisions which the proposed initiative involved, it still appeared to me that great opportunities would be lost unless the problems raised by the atomic development were incorporated into the plans of the allied nations for the postwar world.

This viewpoint was elaborated in a supplementary memorandum in which also the technical problem of control measures was further discussed. In particular, I attempted to stress that just the mutual openness, which now was obviously necessary for common security, would in itself promote international understanding and pave the way for enduring cooperation. This memorandum, dated March 24, 1945, contains, besides remarks which have no interest today, the following passages:

Above all, it should be appreciated that we are faced only with the beginning of a development and that, probably within the very near future, means will be found to simplify the methods of production of the active substances and intensify their effects to an extent which may permit any nation possessing great industrial resources to command powers of destruction surpassing all previous imagination.

Humanity will, therefore, be confronted with dangers of unprecedented character unless, in due time, measures can be taken to forestall a disastrous competition in such formidable armaments and to establish an international control of the manufacture and use of the powerful materials.

Any arrangement which can offer safety against secret preparations for the mastery of the new means of destruction would, as stressed in the memorandum, demand extraordinary measures. In fact, not only would universal access to full information about scientific discoveries be necessary, but every major technical enterprise, industrial as well as military, would have to be open to international control.

In this connection it is significant that the special character of the efforts which, irrespective of technical refinements, are required for the production of the active materials, and the peculiar conditions which govern their use as dangerous explosives, will greatly facilitate such control and should ensure its efficiency, provided only that the right of supervision is guaranteed.

Detailed proposals for the establishment of an effective control would have to be worked out with the assistance of scientists and technologists appointed by the governments concerned, and a standing expert committee, related to an international security organization, might be charged with keeping account of new scientific and technical developments and with recommending appropriate adjustments of the control measures.

On recommendations from the technical committee the organization would be able to judge the conditions under which industrial exploitation of atomic energy sources could be permitted with adequate safeguards to prevent any assembly of active material in an explosive state. . . .

As argued in the memorandum, it would seem most fortunate that the measures demanded for coping with the new situation, brought about by the advance of science and confronting mankind at a crucial moment of world affairs, fit in so well with the expectations for a future intimate international co-operation which have found unanimous expression from all sides within the nations united against aggression.

Moreover, the very novelty of the situation should offer a unique opportunity of appealing to an unprejudiced attitude, and it would even appear that an understanding about this vital matter might contribute most favourably towards the settlement of other problems where history and traditions have fostered divergent viewpoints.

With regard to such wider prospects, it would in particular seem that the free access to information, necessary for common security, should have far-reaching effects in removing obstacles barring mutual knowledge about spiritual and material aspects of life in the various countries, without which respect and goodwill between nations can hardly endure.

Participation in a development, largely initiated by international scientific collaboration and involving immense potentialities as regards human welfare, would also reinforce the intimate bonds which were created in the years before the war between scientists of different nations. In the present situation these bonds may prove especially helpful in connection with the deliberations of the respective governments and the establishment of the control.

In preliminary consultations between the governments with the primary purpose of inspiring confidence and relieving disquietude, it should be necessary only to bring up the problem of what the attitude of each partner would be if the prospects opened up by the progress of physical science, which in outline are common knowledge, should be realized to an extent which would necessitate exceptional action. . . .

In all the circumstances it would seem that an understanding could hardly fail to result, when the partners have had a respite for considering the consequences of a refusal to accept the invitation to co-operate, and convincing themselves of the advantages of an arrangement guaranteeing common security without excluding anyone from participation in the promising utilization of the new sources of material prosperity.

All such opportunities may, however, be forfeited if an initiative is not taken while the matter can be raised in

a spirit of friendly advice. In fact, a postponement to await further developments might, especially if preparations for competitive efforts in the meantime have reached an advanced stage, give the approach the appearance of an attempt at coercion in which no great nation can be expected to acquiesce. . . .

Indeed, it need hardly be stressed how fortunate in every respect it would be if, at the same time as the world will know of the formidable destructive power which has come into human hands, it could be told that the great scientific and technical advance has been helpful in creating a solid foundation for a future peaceful co-operation between nations.

Looking back on those days, I find it difficult to convey with sufficient vividness the fervent hopes that the progress of science might initiate a new era of harmonious cooperation between nations, and the anxieties lest any opportunity to promote such a development be forfeited.

Until the end of the war I endeavored by every way open to a scientist to stress the importance of appreciating the full political implications of the project and to advocate that, before there could be any question of use of atomic weapons, international cooperation be initiated on the elimination of the new menaces to world security.

I left America in June, 1945, before the final test of the atomic bomb, and remained in England, until the official announcement in August 1945 that the weapon had been used. Soon thereafter I returned to Denmark and have since had no connection with any secret, military or industrial, project in the field of atomic energy.

When the war ended and the great menaces of oppression to so many peoples had disappeared, an immense relief was felt all over the world. Nevertheless, the political situation was fraught with ominous forebodings. Divergencies in outlook between the victorious nations inevitably aggravated controversial matters arising in connection with peace settlements. Contrary to the hopes for future fruitful cooperation, expressed from all sides and embodied in the Charter of the United Nations, the lack of mutual confidence soon became evident.

The creation of new barriers, restricting the free flow of information between countries, further increased distrust and anxiety. In the field of science, especially in the domain of atomic physics, the continued secrecy and restrictions deemed necessary for security reasons hampered international cooperation to an extent which split the world community of scientists into separate camps.

Despite all attempts, the negotiations within the United Nations have so far failed in securing agreement regarding measures to eliminate the dangers of atomic armament. The sterility of these negotia-

tions, perhaps more than anything else, made it evident that a constructive approach to such vital matters of common concern would require an atmosphere of greater confidence.

Without free access to all information of importance for the interrelations between nations, a real improvement of world affairs seemed hardly imaginable. It is true that some degree of mutual openness was envisaged as an integral part of any international arrangement regarding atomic energy, but it grew ever more apparent that, in order to pave the way for agreement about such arrangements, a decisive initial step towards openness had to be made.

The ideal of an open world, with common knowledge about social conditions and technical enterprises, including military preparations, in every country, might seem a far remote possibility in the prevailing world situation. Still, not only will such relationship between nations obviously be required for genuine cooperation on progress of civilization, but even a common declaration of adherence to such a course would create a most favorable background for concerted efforts to promote universal security. Moreover, it appeared to me that the countries which had pioneered in the new technical development might, due to their possibilities of offering valuable information, be in a special position to take the initiative by a direct proposal of full mutual openness.

I thought it appropriate to bring these views to the attention of the American government without raising the delicate matter publicly. On visits to the United States in 1946 and in 1948 to take part in scientific conferences, I therefore availed myself of the opportunity to suggest such an initiative to American statesmen. Even if it involves repetition of arguments already presented, it may serve to give a clearer impression of the ideas under discussion on these occasions to quote a memorandum, dated May 17, 1948, submitted to the Secretary of State as a basis for conversations in Washington in June 1948:

The deep-rooted divergencies in attitudes to many aspects of human relationship which have grown out of social and political developments in the last decades, were bound to present a serious strain on international relations at the conclusion of the second world war. While, during the war, the efforts in common defense largely distracted attention from such divergencies, it was clear that the realization of the hopes acclaimed from all the nations united against aggression of a whole-hearted co-operation in true confidence would demand a radically new approach to international relations.

The necessity of a readjustment of such relations was even further accentuated by the great scientific and technical developments which hold out bright prospects for the promotion of human welfare, but at the same time have placed formidable means of destruction in the

hands of man. Indeed, just as previous technical progress has led to the recognition of need for adjustments within civilized societies, many barriers between nations which hitherto were thought necessary for the defense of national interests would now obviously stand in the way of common security.

The fact that this challenge to civilization presents the nations with a matter of the deepest common concern should offer a unique opportunity for seeking continued co-operation on vital problems. Already during the war, it was, therefore, felt that a favourable foundation for later developments might be created by an early initiative aimed at inviting confidence by making all partners aware of the actual situation which would have to be faced, and by assuring them of willingness to share in the far-reaching concessions as to accustomed national prerogatives which would be demanded from every side.

In the years which have passed since the war, the divergencies in outlook have manifested themselves ever more clearly and a most desperate feature of the present situation is the extent to which the barring of intercourse has led to distortion of facts and motives, resulting in increasing distrust and suspicion between nations and even between groups within many nations. Under these circumstances the hopes embodied in the establishment of the United Nations Organization have met with repeated great disappointments and, in particular, it has not been possible to obtain consent as regards control of atomic energy armaments.

In this situation with deepening cleavage between nations and with spreading anxiety for the future, it would seem that the turning of the trend of events requires that a great issue be raised, suited to invoke the highest aspirations of mankind. Here it appears that the stand for an open world, with unhampered opportunities for common enlightenment and mutual understanding, must form the background for such an issue. Surely, respect and goodwill between nations cannot endure without free access to information about all aspects of life in every country.

Moreover, the promises and dangers involved in the technical advances have now most forcibly stressed the need for decisive steps towards openness as a primary condition for the progress and protection of civilization. The appreciation of this point, it is true, underlies the proposals to regulate co-operation on the development of the new resources, brought before the United Nations Atomic Energy Commission, but just the difficulty experienced in obtaining agreement under present world conditions would suggest the necessity of centering the issue more directly on the problem of openness.

Under the circumstances it would appear that most careful consideration should be given to the consequences which might ensue from an offer, extended at a well-timed occasion, of immediate measures towards openness on a mutual basis. Such measures should in some suitable manner grant access to information, of any kind desired, about conditions and developments in the various countries and would thereby allow the partners to form proper judgment of the actual situation that is confronting them.

An initiative along such lines might seem beyond the scope of conventional diplomatic caution; yet it must be viewed against the background that, if the proposals should meet with consent, a radical improvement of world affairs would have been brought about, with entirely new opportunities for co-operation in confidence and for reaching agreement on effective measures to eliminate common dangers.

Nor should the difficulties in obtaining consent be an argument against taking the initiative since, irrespective of the immediate response, the very existence of an offer of the kind in question should deeply affect the situation in a most promising direction. In fact, a demonstration would have been given to the world of preparedness to live together with all others under conditions where mutual relationships and common destiny would be shaped only by honest conviction and good example.

Such a stand would, more than anything else, appeal to people all over the world, fighting for fundamental human rights, and would greatly strengthen the moral position of all supporters of genuine international co-operation. At the same time, those reluctant to enter on the course proposed would have been brought into a position difficult to maintain since such opposition would amount to a confession of lack of confidence in the strength of their own cause when laid open to the world.

Altogether, it would appear that, by making the demand for openness a paramount issue, quite new possibilities would be created, which, if purposefully followed up, might bring humanity a long way forward towards the realization of that co-operation on the progress of civilization which is more urgent and, notwithstanding present obstacles, may still be within nearer reach than ever before.

The consideration in this memorandum may appear utopian, and the difficulties of surveying complications of nonconventional procedures may explain the hesitations of governments in demonstrating adherence to the course of full mutual openness. Nevertheless, such a course should be in the deepest interest of all nations, irrespective of differences in social and economic organization, and the hopes and aspirations for which it was attempted to give expression in the memorandum are no doubt shared by people all over the world.

While the present account may perhaps add to the general recognition of the difficulties with which every nation was confronted by the coincidence of a great upheaval in world affairs with a veritable revolution as regards technical resources, it is in no way meant to imply that the situation does not still offer unique opportunities. On the contrary, the aim is to point to the necessity of reconsidering, from every side, the ways and means of cooperation for avoiding mortal menaces to civilization and for turning the progress of science to lasting benefit of all humanity.

Within the last years, world-wide political developments have increased the tension between nations

and at the same time the perspectives that great countries may compete about the possession of means of annihilating populations of large areas and even making parts of the earth temporarily uninhabitable have caused widespread confusion and alarm.

As there can hardly be question for humanity of renouncing the prospects of improving the material conditions for civilization by atomic energy sources, a radical adjustment of international relationship is evidently indispensable if civilization shall survive. Here, the crucial point is that any guarantee that the progress of science is used only to the benefit of mankind presupposes the same attitude as is required for cooperation between nations in all domains of culture.

Also in other fields of science recent progress has confronted us with a situation similar to that created by the development of atomic physics. Even medical science, which holds out such bright promise for the health of people all over the world, has created means of extinguishing life on a terrifying scale which imply grave menaces to civilization, unless universal confidence and responsibility can be firmly established.

The situation calls for the most unprejudiced attitude towards all questions of international relations. Indeed, proper appreciation of the duties and responsibilities implied in world citizenship is in our time more necessary than ever before. On the one hand, the progress of science and technology has tied the fate of all nations inseparably together; on the other hand, it is on a most different cultural background that vigorous endeavors for national self-assertion and social development are being made in the various parts of our globe.

An open world where each nation can assert itself solely by the extent to which it can contribute to the common culture and is able to help others with experience and resources must be the goal to be put above everything else. Still, example in such respects can be effective only if isolation is abandoned and free discussion of cultural and social developments is permitted across all boundaries.

Within any community it is only possible for the citizens to strive together for common welfare on a basis of public knowledge of the general conditions in the country. Likewise, real cooperation between nations on problems of common concern presupposes free access to all information of importance for their relations. Any argument for upholding barriers for information and intercourse, based on concern for national ideals or interests, must be weighed against the beneficial effects of common enlightenment and the relieved tension resulting from openness.

In the search for a harmonious relationship between the life of the individual and the organization of the community, there have always been and will ever re-

main many problems to ponder and principles for which to strive. However, to make it possible for nations to benefit from the experience of others and to avoid mutual misunderstanding of intentions, free access to information and unhampered opportunity for exchange of ideas must be granted everywhere.

In this connection it has to be recognized that abolition of barriers would imply greater modifications in administrative practices in countries where new social structures are being built up in temporary seclusion than in countries with long traditions in governmental organization and international contacts. Common readiness to assist all peoples in overcoming difficulties of such kind is, therefore, most urgently required.

The development of technology has now reached a stage where the facilities for communication have provided the means for making all mankind a cooperating unit, and where at the same time fatal consequences to civilization may ensue unless international divergencies are held issues to be settled by consultation based on free access to all relevant information.

The very fact that knowledge is in itself the basis for civilization points directly to openness as the way to overcome the present crisis. Whatever judicial and administrative international authorities may eventually have to be created in order to stabilize world affairs, it must be realized that full mutual openness, only, can effectively promote confidence and guarantee common security.

Any widening of the borders of our knowledge imposes an increased responsibility on individuals and nations through the possibilities it gives for shaping the conditions of human life. The forceful admonition in this respect which we have received in our time cannot be left unheeded and should hardly fail in resulting in common understanding of the seriousness of the challenge with which our whole civilization is faced. It is just on this background that quite unique opportunities exist today for furthering cooperation between nations on the progress of human culture in all its aspects.

I turn to the United Nations with these considerations in the hope that they may contribute to the search for a realistic approach to the grave and urgent problems confronting humanity. The arguments presented suggest that every initiative from any side towards the removal of obstacles for free mutual information and intercourse would be of the greatest importance in breaking the present deadlock and encouraging others to take steps in the same direction. The efforts of all supporters of international cooperation, individuals as well as nations, will be needed to create in all countries an opinion to voice, with ever increasing clarity and strength, the demand for an open world.

Technical Papers

Pair Production and Photoelectric Effect in Scintillation Phosphors

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Scintillation counters have often been used as spectrometers to measure the energy of beta and gamma rays. The gamma ray energies have generally been determined by means of the Compton process (1, 3-5). The Compton recoil spectrum is generated largely within the phosphor, resulting in a continuous distribution of electron energies up to a maximum energy determined by the Compton relation $E_{max} = E_\gamma / (1 + E_\gamma / 2E_0)$ where E_γ is the gamma ray energy and E_0 the self-energy of an electron. The gamma ray energy is measured from the more or less sharp high energy edge of the pulse distribution. The lack of sharpness in the pulse distribution is caused by the poor distribution of pulse sizes in the photomultiplier, since different phosphors giving about the same pulse size give about the same spread in pulse size for monoenergetic radiation entering the phosphor.

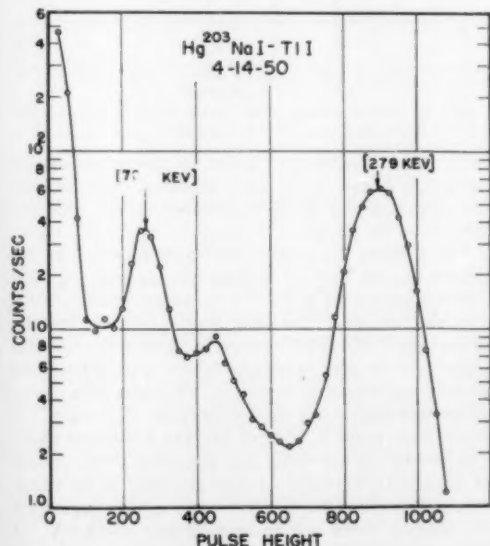


FIG. 1. Photoelectron peaks from the radiations of 51.5 day Hg^{203} .

In the organic phosphors like naphthalene, anthracene, and stilbene, the Compton process is the only interaction of importance for gamma rays from 50 kev to about 3 Mev. Below 50 kev, the photoelectric process becomes important and well-defined peaks or lines of pulses are

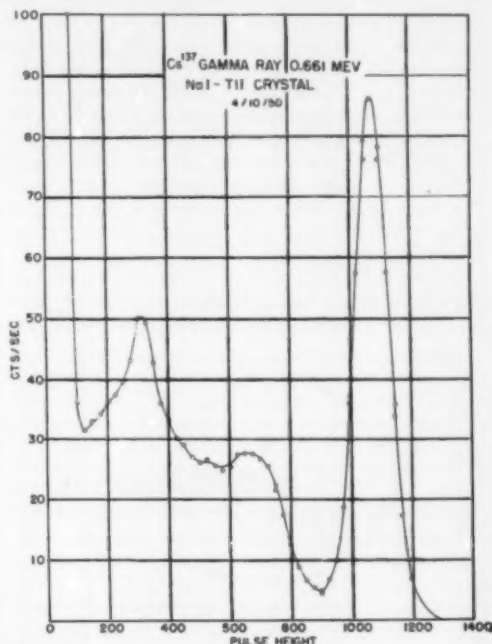


FIG. 2. Linear plot of the pulse distribution produced by 33-year Cs^{137} .

produced. Above 3 Mev, pair production peaks are seen with small intensity.

Sodium iodide activated with thallium (0.5%) is a phosphor producing large scintillation pulses of moderate decay time (0.25 μ sec) and, being largely iodine, interacts with gamma rays largely by the photoelectric process from low energy up to about 1 Mev. Definite lines or peaks in the pulse distribution can be readily observed (2). These peaks have been observed from 25 kev to more than 4 Mev; the pulse height is very closely proportional to gamma ray energy over this whole range and energies can be determined more accurately than from the Compton electron spectrum. These peaks are not found at the gamma ray energy less the K shell binding energy of iodine, as might have been expected, but at the gamma ray energy itself, since the x-rays or Auger electrons from the excited iodine are completely absorbed and restore the whole energy of the gamma ray to the crystal. The figures show the pulse distributions produced in an RCA-5819 photomultiplier at room temperature with a NaI-Tl crystal 1.5 in. in diam and 1 in. thick. Fig. 1 is the pulse spectrum given by Hg^{203} , which has a gamma ray at 279 kev and the Hg K x-ray at 70.8 kev. The Compton electron distribution can be seen below the x-ray peak, breaking off between 450 and 600 divisions of pulse

height. Fig. 2 shows the pulse distribution with a lower amplifier gain for the 0.661-Mev gamma ray of Cs^{137} . Notice that this curve, unlike the rest, is drawn to a linear scale to illustrate the resolution obtainable. The logarithmic plot is generally used, however, to allow all the details of the curve to be seen. The peak at 330 divisions (0.2 Mev) is produced by gamma rays back-scattered from the lead shield. Since the source is near the crystal, these gamma rays must be within ~ 10 degrees from straight backward to reach the crystal, and hence are nearly monochromatic and near the minimum energy for a single Compton scattering (0.184 Mev). The rapid upturn below 100 divisions is due to Ba K x-rays resulting from the large internal conversion of the gamma ray.

As soon as the energy of the incident gamma rays appreciably exceeds 1 Mev, pair production within the phosphor becomes evident, producing other pulse peaks. The

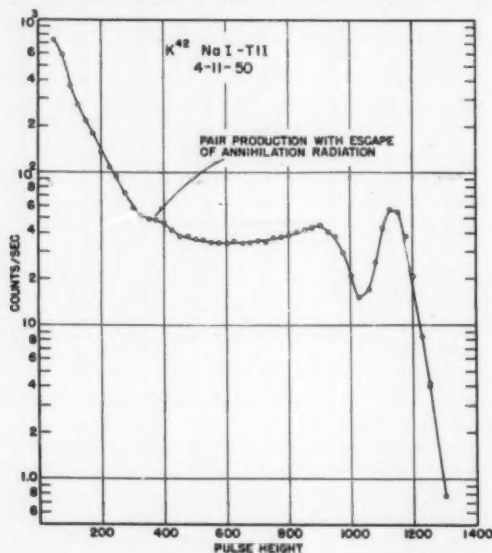


FIG. 3. Compton recoil electrons, photoelectric peak, and weak pair production peak produced by the gamma rays of K^{42} .

gamma ray of K^{42} (1.51 Mev) gives the pulse distribution shown in Fig. 3. The photoelectric peak is considerably smaller with respect to the Compton distribution than in Cs^{137} and a small peak at 385 divisions (0.51 Mev) can be seen. The measured energy difference is 1.00 Mev, which is fairly close to the 1.02-Mev difference that would be expected if this peak represented the kinetic energy of a pair produced by the gamma ray. The peak can be explained if one assumes that the two 0.511-Mev photons from the annihilation of the positron of the pair sometimes escape from the crystal without making any kind of reaction. When one or both photons are detected by the crystal a pulse is produced whose size lies between this low energy peak and the photo

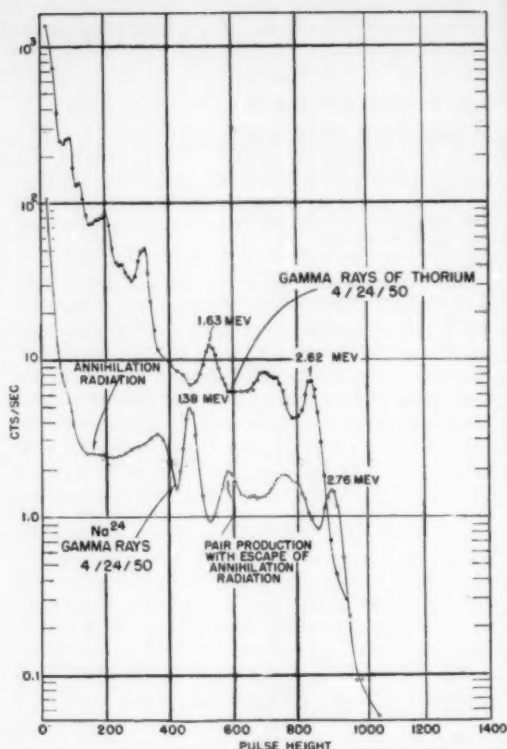


FIG. 4. Pulse distributions for the gamma rays of Na^{24} and Th in equilibrium with its products.

peak. The considerable upturn of the curve at low energies is caused by the many degenerate rays reaching the crystal, as the source was strong and at some distance from the crystal.

Fig. 4 shows the pulse distributions produced by the gamma rays of Na^{24} and thorium and its products. The 2.62-Mev gamma ray of ThC'' gives the three peaks at 2.62, 2.11, and 1.63 Mev. The upper peak is produced both by photoelectric effect with absorption of the x-ray from the iodine and by pair production where both annihilation photons are completely captured. The lower peak is pair production where both photons escaped. The region between these peaks is elevated and has a multiple peak; this seems to be due to the pair production, with capture of one photon superimposed upon the break of the Compton electron distribution. The other gamma rays from the thorium series can be seen, largely unresolved, at lower pulse heights. The two gamma rays of Na^{24} at 1.38 and 2.76 Mev, yield a complicated pattern, as shown in Fig. 4. The Compton recoil electron distribution for the lower energy gamma ray is quite distinct, but the distribution for the upper gamma ray is hidden below the pair production peaks and the broad group of pulses due to partial absorption of the annihilation photons. It cannot easily be determined what part of the peak

at 2.76 Mev is due to pair production and what part is due to photoelectric effect. A rough estimate of the efficiency of the crystal for the annihilation radiation leads to a value of about half photoelectrons and half pair production. The measured energy spacing of the photo and pair peaks gives 0.99 Mev for the thorium curve and 1.00 Mev for the sodium curve. These and other similar values for the self-energy of an electron and a positron are consistently from 2% to 3% below expectation, probably due to the unsymmetrical shape of the lower pair peak. A small peak in the Na^{24} curve at about 0.51 Mev can be seen. This peak is produced by annihilation radiation escaping from the shield wall; it is produced there by the positrons of pairs due to gamma rays from the source that do not enter the crystal. This effect limits the sensitivity with which one can search for annihilation radiation in the presence of high energy gamma rays. The difficulty can be much reduced by using a shield with a liner of low Z material which is thick for annihilation radiation.

With gamma rays of about 7 Mev the lower pair production peak is the most prominent feature of the spectrum. If considerable care is used to make sure of the identity of the peaks, there seems to be no reason why this method of gamma ray measurement could not be extended to much higher energy.

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Phagocytosis during Bacteremia in Mice: A Preliminary Report

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Although the phagocytic property of the neutrophile leukocyte was known to Hayem (13), Panum (22), and Roser (24), it remained for Metchnikoff (20) and his school to emphasize the importance of this cell in normal and pathological physiology. Later investigators, including Denys and Leclef (3), Lelshman (18), and Wright and Douglas (29), employed the neutrophile leukocyte almost exclusively in their studies on phagocytosis. That certain cells of the reticuloendothelial system also possess the same phagocytic property was clearly shown by Werigo (28), Levatidi (19), Tehistovtch (25), Andrews (1), Bull (3-5), Kyes (16), Bartlett and Ozaki (8), Wells (27), Orskov (21), Cappell (6), and Wright (30). Gay and Morrison (11) in their studies on resistance to streptococcal infections even stated that "tissue macro-

phages" are, in large part, if not entirely, responsible for the natural resistance of rabbits to experimental streptococcus infection; this in spite of the obvious presence of polymorphonuclear cells, which have so long been held entirely responsible for the cellular protective mechanism in acute infections (Metchnikoff). Recently, Taliaferro and Mulligan (25) in their important work on defense against malaria also advanced the opinion that resistance is: "essentially a local immunity in strategically placed organs. Phagocytic activity, lymphoid hyperplasia, and the concomitant cytogenesis of macrophages are initiated in the spleen and are always most pronounced in this organ."

In their studies on infections with *Plasmodium cynomolgi* in *Macaca mulatta* they observed: "... an increase in the number of heterophiles (polymorphonuclears) but these cells practically never contain malarial pigment." The same held true in *P. knowlesi* infections.

In the following, an attempt was made to compare the phagocytic functions of the phagocytes in the peripheral blood and of those of the fixed tissues. The work was done in normal and immunized mice subjected to severe bacteremia. Phagocytosis was also determined by the traditional test tube procedure. In addition, the effects of magnesium chloride and gelatin on phagocytosis are herein reported.

Experimental work. The mice were separated into various groups, each comprising animals of similar age and sex and, as nearly as possible, also of similar weight. Prior to the induction of the bacteremia, blood was obtained for total and differential counts. Bacteremia was produced by the intravenous injection of a standardized suspension of a nonpathogenic coccus, *Micrococcus candidus*. Following the injection and at various intervals of time—30 min and 1, 2, 3, 4, 5, and 6 hr—the animals were again bled for total leukocyte and differential counts, for the determination of the percent of active neutrophile leukocytes, and for the number of cocci found per neutrophile leukocyte.

The data thus obtained were treated statistically by the method of Fisher (9), Fisher and Yates (10), or Pearl (23). When the number of animals was greater than 30, the method of the standard error was employed, while for series of less than 30, the Student's T test was applied.

All phagocytic and differential counts were done with complete objectivity. The slides were all recoded before they were examined microscopically.

General observations. Upon injection of the *candidus* into normal mice a drop in the total number of white blood cells occurred in the course of from one to four hours after the injection. This was observed in 49 out of a series of 62 female animals or 79.1 percent. In 22 immunized females, all showed leukopenia in the course of from one to five hours.

In animals sacrificed in the course of the leukopenia it was observed that the neutrophile leukocytes accumulated in their lungs. However, phagocytosis by the mononuclear cells in this area preceded the leukopenia. Large numbers of injected cocci were trapped in the lung.

A study of the neutrophile leukocyte of the peripheral circulation showed that 30 min after the injection of the cecal pabulum relatively few cells acted as phagocytes and that few cocci were ingested. During the leukopenia the number of active cells was at its maximum, and it was at this time that the neutrophiles contained their greatest number of cocci. With the passage of the leukopenia the percent of active cells and the number of cocci per cell reverted to the preleukopenic levels. During the entire course of the experiment, however, there was noted a relative increase in the percent of neutrophiles. In addition, the total leukocyte count increased following the leukopenia; the ultimate effect of these changes led to a rise in the number of cells phagocytizing per cubic millimeter.

The observations described above do not imply that phagocytosis takes place in the peripheral circulation.

The phagocytic cells of the liver, spleen, bone marrow, and axillary lymph nodes were also active in the removal of the injected bacteria but the number of neutrophile leukocytes observed in these organs did not appear to differ markedly from that observed in the control animals.

In animals previously immunized by repeated intraperitoneal injections of the *candidus*, leukopenia occurred more consistently in the course of the first to the third hours. Tissue sections showed that a greater number of neutrophile leukocytes appeared in the lungs of the immunized mice. In the axillary lymph nodes of the immunized animals a greater number of cocci were noted than were seen in the control nodes; these, however, were ingested by the mononuclear cells rather than by the neutrophiles present in the subcapsular areas.

The uptake of the cocci per neutrophile leukocyte was less in the immunized animals than in the nonimmunized controls. In animals showing an agglutinin titer of (1:160 to 1:320) the uptake of the cocci by the neutrophile leukocyte was less than was observed in the controls. In the immunized animals whose antibody titer had fallen in the course of 60 days (no further injections given) the difference became insignificant.

It is possible that the fixed tissue phagocytes of the immunized animal possess greater phagocytic activity than those of the nonimmunized animals. It is therefore not improbable that the same may obtain in the case of the fixed tissue phagocytes.

In both normal and immunized animals, only the mature neutrophile leukocytes exhibit marked phagocytic activity. Participation of the immature cells was negligible. Neutrophile leukocytes with nonsegmented smoothly contoured ring-shaped nuclei were considered immature. In experiments *in vitro*, and with the use of whole blood, however, the immature neutrophile leukocytes proved to be phagocytic and even to contain large numbers of the injected cocci.

Employing a constant number of neutrophile leukocytes and varying numbers of organisms in the test tube procedure, it was found that the degree of phagocytosis depended on the ratio of the number of cocci to the number of neutrophile leukocytes present in the mixture.

This was also observed by Ledingham (17) and by Hanks (18). When the ratio is small (9 cocci to one cell) the difference in the degree of phagocytosis is also small. When the ratio is large (185 cocci to one cell) the difference is markedly reflected in an increase of the total number of organisms ingested. This was also borne out by comparison of the results obtained when NaCl, MgCl₂, NaCl and gelatin, and MgCl₂ and gelatin were used in solutions for suspension of the organisms. When the higher ratios of cocci to cell were used an increase of phagocytosis was also noted. Again, in the test tube procedure, where a high ratio of bacteria to cell was employed, many immature cells participated. This fact gives the impression that both mature and immature neutrophile leukocytes are active in phagocytosis, with the obvious result that the percentage of cells participating would indeed be high. (Ratio of 185 cocci to one cell.)

Gelatin, originally introduced in order to increase the viscosity of the suspension menstuum for the leukocytes, exhibited, rather uniformly, an "opsonin-like" effect. The gelatin employed contained 3.6 µg of magnesium in 0.2 ml of a 1% solution. The blood of the mouse contained about 3.5-4.5 mg %. The determinations were made by the method of Kunkel, Pearson, and Schweigert (16).

Experiments have also been carried out to determine the effect of this cation on phagocytosis. Previous work (7, 14) dealt with its effect on the neutrophile leukocyte in the test tube procedure. In this work, magnesium (MgCl₂) showed a variable effect in the test tube method. In the living animal, however, and in a series of 100 animals (females), there was no significant difference between the percent of neutrophile leukocytes phagocytizing in the control animals and in the animals receiving MgCl₂ (intravenously) up to the third postinjection hour. At the fourth postinjection hour the number of neutrophile leukocytes acting as phagocytes was definitely greater in the magnesium-injected group than in the control group. The probability of statistical reproduction was about 275 to 1. In the case of the number of cocci ingested per neutrophile the probability of statistical reproduction was about 1 in 27.

Elimination of the magnesium ion from the diet of the animals led to a leukocytosis which was noted by the 9th or 10th day. Some of the animals succumbed between the 10th and 14th days. Although the number of cocci ingested per neutrophile leukocyte was no greater in the magnesium-depleted animals, the increase in the number of neutrophile leukocytes per cubic centimeter apparently led to the uptake of a greater number of cocci by these cells. As a result of magnesium depletion the animals lost weight and showed an average total leukocyte count of about 32,000, but with a regular distribution of the white cells.

In conclusion, it may be said that phagocytosis, if it is to be considered from the standpoint of the host, should not be interpreted, as has been done generally, from phagocytic indices derived solely from test tube procedures. An exploration of the phagocytic function

of the reticuloendothelial system is indispensable and should be studied concomitantly.

The experimental details of this work will be published elsewhere.

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A Method for Collecting and Sterilizing Large Numbers of *Drosophila* Eggs

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The techniques used for collecting and sterilizing *Drosophila* eggs, which techniques have been developed mainly for work on the nutritional requirements of the fly (1), proved unsatisfactory when employed by one of us (J. H. S.) for experiments on mutation rates. The sterilization was incomplete when the large numbers of eggs necessary for this work were handled and the labor of removing the eggs individually from the agar media was too time-consuming. An elaboration of the earlier technique was, therefore, developed, and is reported here.

In this context, the most desirable kind of medium for collecting the eggs is one which (1) allows the flies to lay their maximum and (2) can be easily and completely separated from the eggs. The two media listed in Table 1 were found to provide a reasonable compromise between

TABLE 1

CASEIN MEDIUM	
Casein (light white soluble)	1 g
Fuller's earth	1 g
1% Acetic acid in 2% ethyl alcohol	3.0 ml
YEAST AUTOLYZATE MEDIUM	
D.C.L. yeast autolyzate	0.4 g
Fuller's earth	1.8 g
1% Acetic acid in 2% ethyl alcohol	1.0 ml

these necessary characteristics. The casein medium, which is placed in watch glasses and applied to the mouths of the usual *Drosophila* bottles, is usually the more efficient oviposition medium; whereas the yeast autolyzate, which is smeared over a 2% agar gel in watch glasses, is the more readily separated from the eggs. In both cases, the routine procedure is much the same. Eggs are collected for a short period from three- to four-day-old flies (♂) and the surface of the medium is scraped off and dissolved either in 3% sodium bicarbonate (if casein is used) or in water (if the autolyzate is used). The eggs freed by this procedure are then separated from large and small particles by sieving through a coarse and then a fine sieve. The latter has a mesh of 100 to the inch, which is sufficient to retain the eggs but allows the fuller's earth to be washed through.

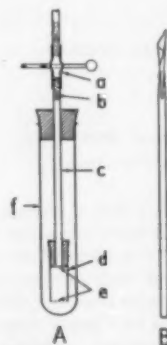


FIG. 1. A, a, Rubber connection; b, cotton wool; c, glass tube; d, glass container for eggs; e, platinum grid; f, outer tube. B, Paper spoon in glass tube.

After washing in running water, the eggs are cleaned by immersion for 10 min in a mixture of 5% antiformin in 10% formalin and are then transferred to the sterilization tube (Fig. 1A) which has been autoclaved prior to use. A series of outer tubes are also autoclaved and filled with the sterilizing fluids. It was found empirically that exposure of the eggs for 20 min to the fluid described in Table 2, followed by 35-min exposure to sterile 70%

TABLE 2
STERILIZING FLUID

HgCl ₂	0.5 g
NaCl	6.5 g
HCl	1.25 ml
Absolute alcohol	500 ml
Water, to	1 liter

ethanol and by 5-min washing in sterile water, gave a high degree of freedom from infection without greatly disturbing the viability of the eggs. Since the eggs are handled in the sterilizing tube, there is little chance of infection from the atmosphere, but as an extra precaution, these operations are usually carried out in a large covered box previously sterilized with an ultraviolet lamp.

The sterile eggs are transferred from the grid of the inner tube (Fig. 1A) by means of sterile paper spoons (Fig. 1B) onto sterile agar plates. The larvae hatch out on the agar and are then picked off with a sterile platinum spoon and placed on the culture media under test. Using this technique it has been possible to set up experiments involving 3,000-5,000 sterile larvae born within 2 hr of each other. When only sterile eggs are necessary, they can be transferred direct from the grid to the medium being studied, and then even greater numbers can be handled successfully.

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Theory of the Electrodeposition of Metals from Aqueous Solutions

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One of the favorite topics of discussion on the results of research is the interpretation of what actually takes place at the surface of the cathode during metal deposition. If we review our findings over a period of almost fifty years, we may summarize as follows:

First, the deposition of a metal at the cathode implies the codeposition of hydrogen. Without hydrogen deposition there is no metal deposition. All metal deposits are crystalline and usually malleable. Amorphous metal deposits at the cathode are due to secondary reactions such as metal oxide to metal.

Second, the hydrogen layer at the surface of the cathode is relatively thin, approximately 0.0001 in. (2.5 micron), and comprises atomic hydrogen, molecular hydrogen, metal hydrides, atomic metal, and an intermediate stage between the metal ion and the metal crystal.

Third, for acceptable metal deposition the cathode surface layer just described must be neither too thick nor

too thin: If the layer is too thick the unit metal crystal is not formed; and if the layer is too thin the discharged metal ion is not sufficiently well protected and no unit metal crystal is formed.

The thickness of an active cathode surface layer may be controlled in several ways:

- a. *By temperature of cathode surface.* The higher the temperature, the thinner is the surface layer.
- b. *By cathode current density.* Within certain limits the higher the cathode current density, the thicker is the layer.
- c. *By mechanical means.* By selecting a cathode which is insoluble in the plating bath and moving it through the bath at a fixed rate, we find that the higher the travel rate of the cathode, the thinner is the cathode surface layer.
- d. *By addition of catalysts to the bath.* Specifically, certain negative ions, such as sulfate ions (which function at the cathode), vary the thickness of the layer.
- e. *By codeposition of a second metal.* The second metal should be comparatively easy to deposit.
- f. *By addition agents electrophoretically deposited at the cathode.* The thinner the addition agent layer, the more metallic is the cathode deposit.

An Assay Method for the Behavioral Effects of L-Glutamic Acid^{1, 2}

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In 1943, Price, Waelsh, and Putnam (9) reported that oral administration of DL-glutamic acid hydrochloride had a palliative effect on patients with petit mal or psychomotor seizures. They also reported improvement in mental and social behavior. Since that time other studies have been made, using subhuman as well as human materials, which confirm these results (1, 13, 14) and which attribute them to the action of the L-isomer (11). For each of these investigations another could be cited in which no beneficial effects of glutamic acid were found under presumably similar experimental conditions (6, 7, 8). The literature on this subject confronts the investigator at one and the same time with theoretical interpretations of facts (12) and with negative data, which make it seem that the facts requiring such interpretation do not exist (10).

One of the major difficulties has been the nonhomogene-

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² This work was done in part at the Division of Behavior Studies, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine, during the summer of 1949. The writers gratefully acknowledge the support of the laboratory.

ity of the populations studied. In the present investigation, genetically homogeneous mice of the dba strain (line 1) were used. These are the result of more than 50 generations of brother \times sister matings and are known to have a quantitatively predictable susceptibility to sound-induced seizures at a given age, when subjected for 2 min to a noise produced by a doorbell mounted on a circular galvanized iron washtub (2, 3, 5). The stimulus intensity is adjusted to approximately 100 decibels with a transformer. When a neutral solution of L-glutamic acid is administered to these animals for 8 to 14 days prior to exposure to audio stimulation, a statistically significant decrease in fatalities resulting from the audiogenic seizures occurs. Under standardized conditions, the magnitude of this decrease may be used to compare the relative effectiveness of various doses alone or with other substances or combinations of substances.

The glutamic acid was administered by subcutaneous injection at a concentration of 200 mg/ml. Each animal received 0.1 ml per 10 g body weight. Solutions were prepared in the manner described by Marx (7). The mice were tested for seizure susceptibility once daily, beginning with the 30th day of age. On these days, the glutamic acid was injected 30 to 45 min prior to exposure to the seizure-producing situation. Thirty-three animals (31 males and 2 females) were used in the control series. Fifty animals (21 males and 29 females) were used in the series receiving glutamic acid. The animals were fed a diet of Fox Chow Checkers and had access to food at all times. The data were tabulated as cumulative percentages for four trials.

A comparison of the treated and control groups reveals that the seizure incidence is not affected by the administration of glutamic acid, but the proportion of fatalities in the treated group is decreased by 18% ($t=2.95$). This effect occurs primarily in males, where the decrease in fatalities below that found in the controls is 26% ($t=3.53$). That for the females is 11%, but is not statistically significant ($t=1.78$). Previous work has shown that untreated males are approximately 10% more susceptible to sound-induced seizures than are females (5). We therefore recommend that dba line 1 males should be used as the standard test animals.

These and other data will be presented and evaluated in greater detail in a forthcoming publication (4).

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The Recombination Coefficient for the F Layer¹

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It seems probable that the time rate of change of maximum electron density does not give a true picture of the actual variations in ionization occurring in the F layer (4). We carried through an investigation to determine whether or not the total electron content of an ionospheric layer can be used for the calculation of α , the recombination coefficient for the region as a whole.

The equation

$$\frac{dN}{dt} = q - \alpha N^2 \quad (1)$$

states that the rate of change of the electron density at any level in the layer is equal to the number of electrons per cubic centimeter produced by any means less the number lost by recombination processes. We must look

into the equation when $\frac{dN}{dt}$ is the rate of change of total electron content in a vertical column of unit cross-sectional area of height τ of the layer, and q is the total production of electrons in this volume. It has been shown that total electron content N_T of the layer below the level of maximum electron density can be computed (3) from:

$$N_T = 2/3\tau N_m \quad (2)$$

As far as q is concerned, we must assume for the present that the sun's ultraviolet light is the only agency responsible for the production of electrons in the layer. Considering only the F₂ layer, the number of electrons recombining with positive ions is proportional to the possible number of collisions of an electron and a positive ion, or

$$R \sim N_e N_p \quad (3)$$

and α can be thought of as a proportionality factor which indicates what proportion of possible recombinations will probably take place. Since we lack precise knowledge of existing conditions, it is convenient to consider that the number of electrons is equal to the number of positive ions, or $R = \alpha N^2$.

For a layer in which the electron density increases with height according to a parabolic law, we have the equation given by Appleton (1, 2),

$$N_h = N_m \left[1 - \left(\frac{h - h_m}{h_m - h_m} \right)^2 \right] \quad (4)$$

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where N_h is the electron density at any height h within the parabolic layer, N_m is the maximum electron density at height h_m , and h_m is the true height of the bottom of the layer. The possible number of combinations of electrons with positive ions at any height within the layer (assuming equal number of electrons and positive ions) is

$$N_h^2 = N_m^2 \left[1 - 2 \left(\frac{h_m - h}{h_m - h_m} \right)^2 + \left(\frac{h_m - h}{h_m - h_m} \right)^4 \right] \quad (5)$$

This must be summed for the half layer from the bottom of the layer to the point where N is maximum, that is, from h_m to h_m . Hence, we have

$$\int_{h_m}^{h_m} N_h^2 dh = \int_{h_m}^{h_m} N_m^2 \left[1 - 2 \left(\frac{h_m - h}{h_m - h_m} \right)^2 + \left(\frac{h_m - h}{h_m - h_m} \right)^4 \right] dh = \frac{8}{15} \tau N_m^2 \quad (6)$$

remembering that $h_m - h_m = \tau$.

The original equation takes the form

$$\frac{dN_r}{dt} = q_r - \alpha N_m^2 \frac{8\tau}{15} \quad (7)$$

The units of α are $\text{cm}^3 \text{sec}^{-1}$ and are the same as with the more simple form of the equation. The α given by this equation is not an α at any particular height but rather an α that describes recombination for the region as a whole. The semithickness τ of the ionospheric layer can be reduced from the original records (4) by the method of Booker and Seaton (5).

	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Mean
$\alpha_m \times 10^{10}$...	3.24	4.87	7.00	5.99	3.48	1.97	4.42
$\alpha_r \times 10^{10}$...	4.54	6.77	11.93	8.37	5.46	2.37	6.57

The recombination coefficients at the level of maximum electron density, α_m , and for the layer as a whole, α_r , were computed from night ionospheric data observed at College, Alaska. During the night q vanished from the

equation. The monthly mean data for the winter months (October through March) of 1948-49 were used for the calculations. The mean results are tabulated in Table 1.

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Lipid Interrelationship in Health and in Coronary Artery Disease

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It has been demonstrated that the serum cholesterol level is elevated in atherosclerosis and coronary artery disease (4, 9). Recently it has been suggested that an inverse correlation exists between serum cholesterol and time of appearance of atherosclerosis (8). Thus, individuals experiencing such diseases at an early age would be expected to show highly elevated serum cholesterol levels (over 300 mg %). This relationship has been under investigation for the past three years by the Coronary Research Project at the Massachusetts General Hospital.

In keeping with other reports, the present study found that serum cholesterol was considerably higher in males

who had experienced myocardial infarction prior to the age of 40 than it was in healthy, active males of comparable age, the means being 286 ± 6.6 mg/100 ml blood and 224 ± 3.5 mg/100 ml blood, respectively, the difference exceeding one standard deviation of the normal group (6). Even though individual thresholds may exist, there was no evidence of a threshold "value" of serum cholesterol in the coronary disease group, the distribution being essentially continuous.

On further analysis of other serum lipids, it was found that the normal interrelationships of these lipids were altered in coronary artery disease as reported recently (1, 7). Since these observations also indicate that relationships rather than absolute serum levels are important, this communication includes a study of such relationships.

In this study, blood samples were taken from 243 individuals; 97 were males who had experienced myocardial infarction prior to the age of 40, and 146 were healthy, active working males comparable in age and other variables. Serum cholesterol determinations were made using the method of Bloor (2), while serum phospholipid determinations were made by the Fiske and Subbarow method (5). The two groups of individuals are referred to as the coronary artery disease group and the control group hereafter.

Results giving the mean values, standard deviations, and standard errors of the two lipids, and their ratios are summarized in Table 1.

The serum cholesterol and serum phospholipids means

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TABLE 1
SERUM CHOLESTEROL, SERUM PHOSPHOLIPIDS, AND CHOLESTEROL/PHOSPHOLIPID RATIO IN THE CONTROL GROUP
AND THE CORONARY ARTERY DISEASE GROUP

	Serum cholesterol*		Serum phospholipids*		Ratio: $\frac{\text{cholesterol}}{\text{phospholipids}}$	
	control group	coronary disease group	control group	coronary disease group	control	coronary
Number	146	97	146	61	146	60
Range	148-332	167-490	215-415	195-414	52.0-104.0	60.4-119.8
Mean \pm S.E.†	224.4 \pm 3.5	286.5 \pm 6.6	209.3 \pm 3.3	316.4 \pm 6.6	75.1 \pm .02	89.4 \pm 2.04
S.D.‡	42.6	64.9	40.2	52.2	10.9	15.9

* Figures represent mg/100 ml blood. Serum phospholipids are expressed as Helthin (25 \times lipid phosphorus).

† S.E. = Standard error.

‡ S.D. = Standard deviation.

were both significantly higher in the coronary disease group, while the ratio of cholesterol/phospholipids was also significantly higher in the coronary disease group. Proportionately, the cholesterol level had risen higher in the coronary artery disease group than in the control group. It is reasonable to assume that if serum phospholipids were to rise proportionally to the serum cholesterol, the ratio would remain unchanged. Thus, it is obvious that the ratio is increased in the coronary disease group due to the lack of a proportional rise in serum phospholipids.

The correlations between the two lipids and between age and each of the lipids were determined for both the coronary disease group and the control group, as shown in Table 2.

Thus there is, in the normal control group, a moderate correlation between the two lipids, and low to moderate correlations between age and each of the two lipids studied. However, in all three correlations the coronary disease group is significantly lower, with the correlations between age and cholesterol and age and phospholipids no longer significantly different from zero. This is further proof that the interrelationship between the lipids

and phospholipids remains moderately high in both groups, although it is lower in the coronary disease group. The serum phospholipids continue to show an age correlation in the normal group, but it is an insignificant one in the coronary disease group. Serum cholesterol, on the other hand, failed to show age changes with serum phospholipids held constant in both groups. Thus, again the normal age increments are absent in the coronary disease group, while phospholipids seem to mediate the normal cholesterol-age relationship in some way in the normal.

It is therefore reasonable to suggest that the phospholipids play a role in the normal age changes in serum cholesterol; the failure to find such cholesterol changes in coronary artery disease may merely reflect the basic difference in amount and in proportions of the phospholipids in this disease group.

Peters and Man (10) suggested that the interrelationships of the serum lipids are far more important than the consideration of any single lipid. The protective ac-

TABLE 3
PARTIAL CORRELATIONS: AGE, CHOLESTEROL, AND
PHOSPHOLIPIDS

Partial correlation	Control group	Coronary disease group
Cholesterol and phospholipids, age constant	+ .62 \pm .05	+ .50 \pm .10
Cholesterol and age, phospholipids constant	+ .05 \pm .08*	+ .07 \pm .10*
Phospholipids and age, cholesterol constant	+ .32 \pm .07	+ .14 \pm .13†

* Not significantly different from zero.

* Not significantly different from zero.

† Not significant.

is disturbed in coronary artery disease, while the normal age trend is largely masked if not disrupted.

Because of the number of intercorrelating variables at work in the two groups, partial correlations were calculated (Table 3) in order to study the effects of two intercorrelating variables, with the third held constant. Accordingly, each of the three variables was eliminated in turn.

With age constant, the correlation between cholesterol

tion of serum phospholipids in experimentally produced atherosclerosis has recently been demonstrated, and its method of protection has been speculatively described as being in the nature of a colloid stabilizer (1, 7). This is in keeping with Browder's earlier observation of the antagonistic effect of serum cholesterol and serum phospholipids in biological reactions (3).

From the results of this general study, it is known

that the serum-cholesterol in individuals with coronary artery disease reaches inordinately high levels in many (but not all) instances. The serum phospholipids, on the other hand, do not keep pace with this rise in serum cholesterol. Hence it is believed that one of the factors favoring the deposition of cholesterol in the intima is enhanced because of the lack of a colloid stabilizer which may be reflected by the proportion of phospholipids in the serum. Conversely, in the normal individual it may be suggested that the colloid stability of cholesterol is unchanged because the rise of serum phospholipids is proportional to the rise in serum cholesterol.

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Inhibition of Anaphylaxis in Guinea Pigs by D-Catechin

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The use of antihistaminic agents has proved to be an effective adjunct in the treatment of various allergenic reactions by virtue of their antagonistic activity toward preformed histamine. Recently, Martin *et al.* (4) demonstrated *in vitro* the inhibitory effects of vitamin P compounds in histidine decarboxylase. This enzyme, present in animal tissues, is capable of forming histamine from histidine (3, 6, 7). Preliminary tests *in vivo* (1) also indicated that these compounds are active. Their activity might be directed toward inhibition of the formation of histamine. Inhibition of histamine formation in the body seems a rational approach to the treatment of allergies.

In this study, 14 guinea pigs were sensitized in the manner described by Raiman *et al.* (5). Half of the animals received 2 mg of D-catechin, an aglycone flavonoid, intraperitoneally daily for 19 days. The remaining animals were not treated and served as controls. At the end of the 19-day period each animal was shocked by an intracardial injection of 0.1–0.5 ml of fresh normal horse serum.

The animals receiving D-catechin exhibited no anaphylactic reactions. The control animals exhibited typical reactions followed by extreme dyspnea and finally death due to asphyxia. The complete reaction lasted approximately 5 min.

Four additional guinea pigs, which had received daily doses of D-catechin for 1 week, were injected intracardially with 0.1 mg of histamine diphosphate. These animals died several minutes later with typical shock symptoms.

The dead control animals and the animals from the histamine group were autopsied. No significant difference in gross pathology could be observed. The predominating characteristic in both groups of animals was the constriction of the bronchiolar muscles. Each animal showed varying degrees of pulmonary edema and hyperemia.

These studies show that D-catechin protects guinea pigs from anaphylactic reactions but not from histamine shock. It appears reasonable to believe that this protective activity might be attributed to an actual inhibition of histidine decarboxylase. This reaction would tend to prevent the formation of histamine, which is an important factor in the anaphylactic syndrome (2).

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Regeneration of the Shoot Apex of *Lupinus albus* after Operations upon the Central Initials

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In attempting to transplant the central portion of the shoot apex, it was noted that the uninjured portions of the original meristem regenerated into one or two normal apices. This regeneration was similar to that described by Linsbauer (2) and Pilkington (3) after different operations. The heavy black line in Fig. 1 shows the position of the cuts made in the shoot apex. The sector (8) was either transplanted to another apex, replaced in the same or reversed orientation in the original apex, or excised. Usually the sector died when it was left in an apex (Figs. 2, 3, 4, 5), and its shrunken remains marked the site of the operation. When the sectors were re-

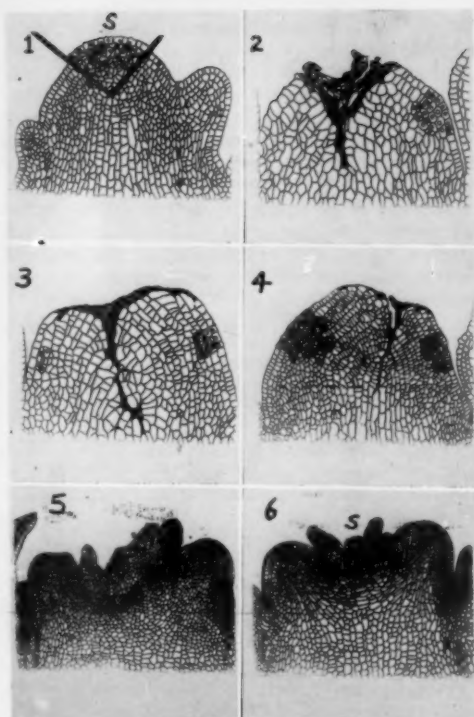


FIG. 1. Longitudinal section diagram of the shoot apex showing, with the heavy black lines, the position of the cuts made to take out the sector (S) containing the central initials (with outlines of nuclei). Magnification $\times 100$.

FIG. 2. Diagram of a shoot apex two days after the operation. The sector had shrunk and died. New initials occur in the right regenerating apex. Magnification $\times 148$.

FIG. 3. Shoot apex four days after the operation. The sector is further shrunk. New initials are seen in both right and left regenerating apices. Magnification $\times 130$.

FIG. 4. Shoot apex six days after the operation. Large groups of initials occur in both regenerating apices. Magnification $\times 95$.

FIG. 5. Regenerated apices 18 days after the operation. The apices had produced shoots 90° from each other, leaving behind the remains of the sector. Magnification $\times 43$.

FIG. 6. Regenerated apices 18 days after the operation. The apices had produced shoots 90° from each other, leaving behind the sector (S), which had united with the subjacent tissues. Magnification $\times 55$.

placed in the same orientation in the original apices, a very few of the sectors united with the subjacent tissues (S in Fig. 6). Only in this limited degree were transplants successful. In such successful grafts the sector was not retained as a part of the shoot apex, but was left behind by the growth of the regenerated apices. Irrespective of what was done with the sector, or whether it did or did not unite with the apex, the uninjured re-

mains of the original meristem underwent regeneration by forming new initials in lateral positions (Figs. 2, 3, 4). It had previously been postulated (1) that the cells along the flank of this apex are equivalent anatomically to the initials. This mode of regeneration demonstrated



FIG. 7. Plant with regenerated shoots 61 days after the operation. The new shoots were approximately parallel to the original shoot and to each other. Magnification $\times \frac{1}{2}$.

that the cells along the flank of this shoot apex, although they produce foliar primordia in the original apex, could, under the circumstances of this experiment, become the central initials of a new shoot apex. The flank cells are therefore equivalent morphogenetically to the original central initials. The origin of the new group of initials appeared to entail periclinal divisions of the second tunica layer (Figs. 2, 3). Derivatives of the tunica were therefore contributed to the new corpus. The shoot apex, in its regeneration, appeared to function as a unit, and to determine how the individual cells would divide, and what they would become. The early growth of the regenerated apices occurred away from each other. The axes of the new shoots in Figs. 5 and 6 are approximately 90° from each other and 45° away from the axis of the original shoot. It is not known whether the angles of the new shoots were due to mutual repulsion of adjacent centers of growth, or merely due to the position of their origin on the sides of the original meristem (Figs. 2, 3, 4). Further studies are being made with the hope of obtaining evidence on this matter. After the regenerated shoots attained considerable length, they no longer grew at a sharp angle from each other, but were approximately parallel to each other and to the original shoot (Fig. 7).

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Studies in Edema: Cholesterol and Its Relation to Protein Nitrogen in Edema Fluid

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The only study of cholesterol in edema fluid has been that of Chauffard *et al.* in 1911 on four patients (4). We report here 16 cases and include the correlation of cholesterol with the protein nitrogen content of edema fluid.

Material and Methods. The patients studied fell into two classes (clinical details in Table 1): (a) those with

(b). Nitrogen determinations were performed by the micro-Kjeldahl method (7).

Results. Results are presented in Table 1.

Discussion and Conclusions. The average edema fluid cholesterol of 14.6 mg % in congestive heart failure is in sharp contrast to the average of 175.2 mg % in lymphatic obstruction. This may serve to differentiate these fluids.

Chauffard *et al.* reported cholesterol values (method not specified) in edema fluid from two patients with Bright's disease, (5.0 and 3.0 mg %) and two with cardiac edema (1.3 and 4.5 mg %). Protein nitrogen was not investigated.

TABLE 1
CHOLESTEROL AND PROTEIN NITROGEN IN EDEMA FLUID AND SERUM

Patient	Clinical			Type of edema*	Total cholesterol			Protein nitrogen† mg %		Cholesterol ester mg %	
	Age	Sex	Diagnosis*		Edema mg %	Serum mg %	$\frac{E}{S} \times 100$	Edema	Serum	Edema	Serum
S.P.	80	M	Anemia ACVD	C.H.F.	19.4	161.0	12.1				
R.I.	62	F	HCV	C.H.F.	27.1	524.0	5.2	46.4	754.0	8.0	418.0
A.E.	57	F	HCV	C.H.F.	25.0	244.0	10.2	87.0	1034.0		
A.L.	52	F	HCV	C.H.F.	7.8	302.0	2.6	45.7	1002.0		
M.W.	54	F	RHD	C.H.F.	6.1	187.0	3.3	44.6	1058.0	0.0	117.0
A.B.	50	F	RHD	C.H.F.	2.4	163.0	1.5	27.6	940.0	1.0	181.0
T.T.	60	F	Ca.	V.O.	30.0	215.0	14.0				
L.K.	61	M	Uterus Ca.	V.O.	9.6	364.0	2.6				
V.G.	44	M	Colon	V.O.	12.3	277.0	4.5	89.8	958.0	0.0	111.5
H.A.	75	M	Ca. Rectum	L.O.	102.0	213.0	77.4				
M.P.	51	F	Ca. Breast	L.O.	151.5	322.0	45.7	580.0	1170.0	92.5	245.5
L.J.	71	F	Ca. Breast	L.O.	212.0	421.0	50.2	712.0	1284.0		
M.D.	44	M	L.S.	Stasis	21.0	245.0	8.6				
M.J.	24	F	K-W.	Hypoproteinemia	18.8	552.0	3.4				
M.G.	62	F	Ca. Breast	C.H.F. (?)‡	7.2	382.0	1.9	25.8	830.0	0.0	166.0
T.L.	62	F	Ca. Colon	C.H.F. (?)‡	15.2	442.0	3.4	42.8	866.0	12.8	336.0

* ACVD arteriosclerotic cardiovascular disease; HCV hypertensive cardiovascular disease; RHD rheumatic heart disease; Ca. cancer of; L.S. lateral sclerosis; C.H.F. congestive heart failure; V.O. venous obstruction; L.O. lymphatic obstruction; K-W. Kimmelstiel-Wilson's syndrome.

† Total nitrogen mg % minus 30 mg % nonprotein nitrogen.

‡ Not included in average values for congestive heart failure.

edema originating from congestive heart failure, and (b) those with edema of an obstructive origin, either venous or lymphatic.

Edema fluid was obtained by inserting an 18-gauge needle subcutaneously into the extremity and letting the fluid drip into a test tube. Venous blood was drawn simultaneously.

Total cholesterol and cholesterol esters were determined in serum and edema fluid by Bloor's method (1).

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The total cholesterol in edema fluid is related to the protein nitrogen content (coefficient of correlation, $r=0.99$). The average total cholesterol/protein nitrogen ratio in edema fluid is 0.26 ± 0.14 . This relationship of cholesterol to protein nitrogen in edema fluid confirms that found in other pathological body fluids (3, 5, 6).

In summary, cholesterol content of edema fluid in patients with congestive heart failure averaged 14.6 mg % (average edema fluid/serum cholesterol ratio was 6%). In edema fluid originating from venous obstruction, the average cholesterol content was 17.3 mg % (average

edema fluid/serum cholesterol ratio was 7%). Where lymphatic obstruction was the predominant factor in edema formation, the cholesterol content averaged 175.2 mg % (average edema fluid/serum cholesterol ratio was 58%). The average total cholesterol/protein nitrogen ratio in edema fluid is 0.26 ± 0.14 .

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The Urea Complexes of Unsaturated Fatty Acids

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The phenomenon of urea complex formation with aliphatic straight chain compounds was discovered in 1940 by F. Bengen.¹ He showed that normal aliphatic compounds form complexes with urea by addition, whereas branched and cyclic compounds do not, thus allowing the separation of straight chain compounds from the others by complex formation. Bengen and Schlenk, in a preliminary report emphasizing that the complexes formed are of a type so far unknown (1), recently summarized research in this field during the intervening years. Zimmerschied *et al.* called attention to, confirmed, and extended the observations in the original patent application (3, 4). Schlenk reports in detail on the formation of urea complexes in relation to the shape of the organic molecule, their composition, their crystal structure, and their energy of formation (2).

These reports emphasize that to form urea complexes, straight chain molecules are required. It was of interest to learn what influence the shape of unbranched molecules has upon the urea addition. For this study the unsaturated fatty acids of the C_{18} series were chosen because the double bonds alter the shape of the molecules. In general, it was found that the unsaturated fatty acids form urea addition complexes also. The degree of unsaturation, the position of the double bonds, and *cis-trans* isomerism do not markedly influence the composition of the complexes, 14.0–14.5 moles urea per mole C_{18} acid.

A remarkable property of the complexes of the unsaturated acids is their resistance to autoxidation. This is illustrated in Fig. 1. In another experiment with

¹ German patent application O.Z. 12438 (March 18, 1940): Technical Oil Mission Reel 6 frames 263–270 in German, Reel 143 pages 135–139 in English.

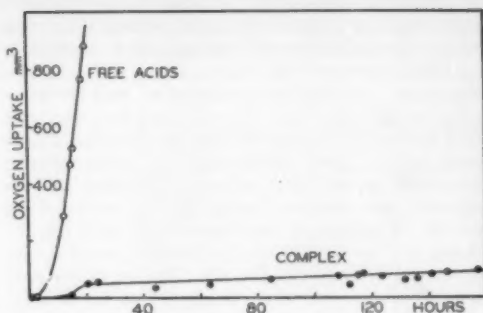


FIG. 1. Oxygen absorption of soybean fatty acid complexes and their freed acids in the Warburg respirometer at 37° under air. Samples: 400 mg complex, 90 mg freed acids.

larger quantities, the autoxidation of soybean fatty acids and their urea complexes was followed for several weeks by means of their peroxide contents (Table 1). From these experiments, performed in October, 1949, it is apparent that the unsaturated fatty acids are inaccessible to oxygen in the form of complexes. This is understandable from the crystal structure of urea addition complexes (2).

TABLE 1
PEROXIDE VALUES OF SOYBEAN FATTY ACIDS AND THEIR COMPLEXES EXPOSED TO AIR AT ROOM TEMPERATURE

	Weeks			
	0	1	2	3
Free acids	1	82	193	260
Complex acids	1	..	6	3

TABLE 2
ENRICHMENT OF THE SATURATED AND UNSATURATED COMPONENTS OF FATTY ACID MIXTURES

Fatty acids		Urea	Complex acids	Non-complex acids	
Source	g	g	Yield, %	I.V.*	Yield, %
Soybean, I.V. = 141 ...	100	30	9	56	81
Soybean, I.V. = 141 ...	100	100	37	88	56
Soybean, I.V. = 141 ...	100	200	67	119	27
Chinese tallow, I.V. = 19	51	100	27	6.5	18.5
Olive, I.V. = 80	50	15	5.5	54	36

* I.V. = Iodine value.

Although all normal saturated and unsaturated fatty acids thus far investigated form urea addition complexes, the yields under identical conditions vary widely. This can be caused by differences in the relationships:



and can be used as a basis of separation of various types

of unbranched compounds. A series of experiments in which urea was added in amounts insufficient for total complex formation is summarized in Table 2. Under such conditions, saturated long chains combine with urea preferentially. In another experiment soybean oil fatty acids having iodine value 141 were separated into fractions having iodine values 86, 148, 181, and 200. In the same manner, other enrichments have been achieved. Autoxidized soybean fatty acids, peroxide value 33, were separated into fractions having peroxide values of 15 and 86. A mixture of lauric and stearic acids (50/50) having acid number 240 was fractionated to acid values of 207 (12/88) and 266 (83/17). Similar experiments have shown that urea complexes can be used for the separation of normal aliphatic compounds of different chemical character.

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An Ideal Preparation for Dissection of Spinal, Peripheral, and Autonomic Nerves of the Rat¹

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In the normally nourished rat, the presence of large amounts of fat and bulky opaque muscle makes it very difficult or impossible to dissect out any except the largest nerves; furthermore, it is often difficult to distinguish glandular from fatty tissue. Ordinary starvation does not remedy this situation, since in the 3-5-day period that a rat survives without food, only a small amount of fat and very little muscle tissue is lost.

Only recently it was found that the method used in nutritional experiments carried on for many years in this laboratory with the so-called "single-food choice" diets (3) may provide ideal preparation for dissection of nerves and for differentiation between fat and glandular tissue in the rat. In the simplest form of these experiments, rats of a standard age and weight are kept on a diet limited to water and one foodstuff (for instance, dextrose, sucrose, olive oil, butter, casein, or lactalbumin) and the survival-times are taken as a measure of the nutritional value of the foodstuff. On dextrose the rats live on the average 37 days—that is, 33 days longer than on no food at all. In a more complicated form of these experiments the rats have access to water and one foodstuff and also to a supporting substance: for instance to a single purified food, such as dextrose, and to a supporting substance such as thiamine. The increase in sur-

¹ Carried out under a grant from the Corn Industries Foundation, New York City.

vival time over that obtained on the single food alone gives a measure of the part played by the supporting substance in the utilization of the foodstuffs. For example, on dextrose and with access to thiamine the rats live 76 days, over twice as long as on dextrose alone, thus giving a dramatic demonstration of the part played by thiamine in the utilization of dextrose (4). In slightly more complicated experiments the rats have access to a combination of foodstuffs, such as a solution of dextrose (15%) and alcohol (15%). On this diet the rats lived on the average 37 days, and with access also to a thiamine solution, 55 days.

Most of the specimens used for dissection of nerves and glands were rats that had been on the dextrose-alcohol-thiamine diet for 40-60 days. Specimens obtained with a diet of dextrose or sucrose, and thiamine (without alcohol) would have served just as well.

Of interest for the present purpose is the fact that on these single-food-choice diets the rats continue to live for a long period of time, lose weight at a slow rate, and after 40-60 days show no symptoms of nutritional deficiency except emaciation. Their teeth, skin, hair, and bones appear normal; none of the internal organs shows any lesions. However, the changes that result from emaciation make them ideal specimens for dissection. Not one trace of fat remains; most of the muscles are so thin and transparent that the underlying tissues may clearly be seen through them (for example, the lungs are visible through the muscle walls of the thorax); the cranial and sacral autonomic nerves, the sympathetic nerves and rami, and the spinal nerves stand out clearly without any obstruction; the glands of internal and external secretion are at least as large as in normal rats of the same size.

Special use of these prepared specimens has been made for the differentiation between true fat and tissue that often may be mistaken for fat. For example, the rat has deposits of so-called brown "fat" in several locations on the body—between the shoulder blades, retroperitoneally and retrothoracically along the spinal column, and near the salivary glands (1). In a normally nourished rat this brown fat can be distinguished from the surrounding fat, but often with some difficulty. In the partially starved rats the brown fat persists long after all regular fat has gone, and its dark red-brown color stands out sharply against the muscles. The response of this tissue to partial starvation is entirely different from that of regular fat, so that its designation as fat is probably a misnomer and its designation as a gland (hibernating?) may be more correct.

Glands of internal as well as of external secretion, such as the preputial glands that may be mistaken for fat, can also be clearly differentiated from fat by this method. The absence of fat surrounding the glands in these cases makes it possible to distinguish all the autonomic nerves that lead to them.

One of the most striking effects of the single food diet is the complete arrest of bone development. E. A. Park, who is making a study of the bones in these rats, states that the arrest is more complete than he has ever observed.

At present he is using this arrested growth as a base line, adding nutrients one by one and determining which will renew bone growth.

We have not as yet tried the single-food-choice technique on larger animals, such as the dog, cat, or monkey. Some nutritional observations made in 1816 by Magendie (2) suggest that it may work quite as well on dogs and within much the same time limits. Magendie found that dogs kept on an exclusive diet of butter or sugar or gelatin lived 30-36 days, which is about the same length of time that rats live on these foodstuffs alone.

In summary, a method of prolonged partial starvation has been described which provides ideal preparation for dissection of spinal, peripheral, and autonomic nerves of the rat; also for the differentiation between glandular and fatty tissue.

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Fibrinolytic Activity of Purified Thrombin¹

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Highly purified thrombin at a concentration of 9,000 units per ml will completely dissolve a 0.5% fibrin clot at 37.5° C within a 90-min period. Evidence that thrombin is responsible for dissolution of the clots is based upon the degree of purification of thrombin preparations, failure of prothrombin from which the thrombin was derived to lyse fibrin clots, equal ability of thrombin prepared by two different methods of activation to lyse fibrin, relative lack of inhibition of fibrinolysis in the presence of antifibrinolysin (antiplasmin) or soybean antitrypsin, and heat inactivation of thrombin which parallels the decrement in lytic activity.

Reagents used in fibrinolytic tests consisted of fibrinogen substrate, prothrombin or thrombin buffered with imidazole, and in some experiments antifibrinolysin or soybean antitrypsin. Fibrinogen, prepared from bovine plasma by the freeze-thaw technique (21) was free of demonstrable fibrinolysin (plasmin) and profibrinolysin (plasminogen). Prothrombin was prepared from bovine plasma by techniques which have been described (20, 22). These products had specific prothrombin activity values ranging from 1,200 to 1,400 units³ per mg of dry weight. Thrombin was prepared from prothrom-

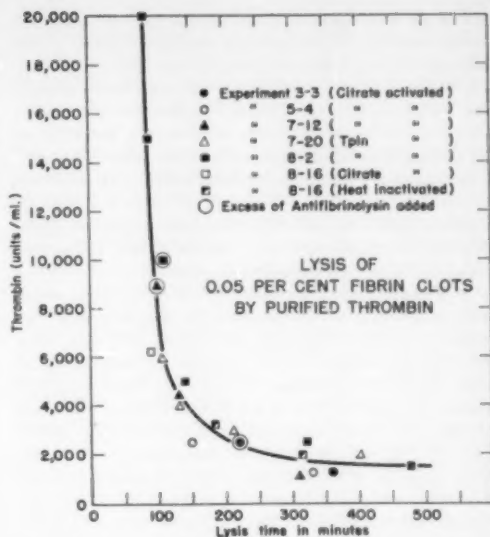


FIG. 1.

bin by activation with citrate or thromboplastin (18, 20, 22). Reactants in the lytic tests were buffered at pH 7.2 with 1% imidazole. At this concentration imidazole has been found to be strongly bacteriostatic (6).

All tests for fibrinolytic activity were carried out at 37.5° C. Fibrin clots were formed in test tubes approximately 7 mm inside diam and 75 mm in length. To 0.2 ml of a 0.1% fibrinogen solution was added an equal volume of the substance to be tested. Lysis was considered to be complete when material in the tube flowed freely as the tube was tipped with the open end at an angle of 10° below the horizontal. At the point of complete lysis the solutions became clear and nonviscous. After the lysed solutions had stood for varying periods of time a flocculent precipitate appeared. The nature of this precipitate has not been investigated.

The plot on the graph shown in Fig. 1 results in deviation from the smoothed curve within the limits of error of the analytical methods used. Each experiment, indicated by a distinctive symbol, represents a single thrombin preparation which was used in its most concentrated form and in one or more dilutions.

All prothrombin preparations were checked for fibrinogenolytic and fibrinolytic activity prior to conversion to thrombin. Fibrinogen solutions containing prothrombin were clotted solidly by addition of thrombin after 72 hr of incubation. Similar solutions, to which a trace of thrombin was added at the start of incubation, clotted but did not lyse. These results indicate that prothrombin does not have a measurable lytic action on either fibrinogen or fibrin.

Since prothrombin preparations from which thrombins were derived had no fibrinolytic activity, it is evident that the lytic property appeared as a result of prothrombin activation. Two methods of activation were used.

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³ The unit of prothrombin and the unit of thrombin used in this work are those described by Ware and Seegers (24).

Maximum conversion of prothrombin to thrombin with citrate occurred within 24 hr at room temperature in the presence of a high concentration of sodium citrate. Thrombin was then precipitated with ammonium sulfate, and the precipitate was dialyzed against deionized water at pH 7. Thrombin was also prepared by the addition to prothrombin of calcium and resedimented thromboplastin, free of fibrinolytic contaminants. As indicated on the graph, the lytic activity of thrombin prepared by either method was essentially the same, which suggests that the lytic property is independent of the method and reagents used in the conversion process.

In three different thrombin preparations lysis times were determined in the presence of approximately 240 units of bovine antifibrinolysin (12).⁴ This concentration of antifibrinolysin will inactivate 24 units of fibrinolysin within about 15 min at 28° C, and 1 unit of fibrinolysin will lyse 1 ml of a 0.1% fibrin clot at 28° C in 120 sec in an isotonic saline solution buffered with imidazole (4). No difference in lysis times could be detected, in the presence of equal thrombin concentrations, between the tubes containing antifibrinolysin and those not containing antifibrinolysin. The failure of antifibrinolysin to inhibit lysis of the clot furnishes evidence that the lysis is not caused by contaminating fibrinolysin.

In one experiment soybean antitrypsin,⁴ in sufficient concentration to completely inactivate 10 mg of crystalline trypsin (11), was dissolved in the fibrinogen solution before adding thrombin. Thrombin activity in this test was 9,000 units per ml. Since thrombin activity is roughly 1,000 units per mg of the dry solid, antitrypsin added was in excess of the amount required to inactivate all of the thrombin if it were assumed to be crystalline trypsin. The clot containing antitrypsin completely lysed in 375 min, while the clot containing the same amount of thrombin but free of antitrypsin lysed in 100 min. The partial inhibition of lysis which occurred was probably not specific or lysis would have been completely prevented. The partial inhibition is more likely associated with some of the other effects which soybean antitrypsin appears to have on organic substrates in addition to its inactivation of trypsin (3, 5).

Purified thrombin in saline solution is inactivated at a measurable rate by heating at 50° C (17). One thrombin preparation was tested for lytic activity prior to heating and after heating to 50° C for 15 and for 30 min. The unheated material contained 12,500 thrombin units per ml. After heating for 15 min, 6,560 units remained; while after heating for 30 min, 3,900 units remained. The plot of these points on the graph follows the dilution curve, indicating that the decrement in thrombin activity is equivalent to the decrement in lytic activity as measured by the method of dilution. Since it is improbable that a contaminating lytic principle and thrombin both have the same rate of heat inactivation, the experiment indicates that fibrinolytic activity resides in thrombin itself.

Our data indicate that thrombin will produce complete

⁴Kindly supplied by E. C. Loomis of Parke, Davis and Company.

lysis of a clot within 1 to 4 hr only if its concentration is roughly equal to that of the fibrin. Earlier workers (1, 7, 9, 14, 15), who have reported that thrombin has the ability to redissolve the blood clot, did not have available sufficiently potent thrombin to lyse fibrin clots. Since purified thrombin has been available only recently, it is probable that thrombin preparations of the workers cited contained fibrinolytic contaminants which caused the described lysis. Seegers (17) and Hudemann (8), in reporting that thrombin does not have fibrinolytic activity, had preparations which were not contaminated but the thrombin was not sufficiently concentrated to cause dissolution of the clots.

In addition to its action on fibrinogen and on fibrin, there is evidence that thrombin also affects other substrates. Thrombin can either inactivate prothrombin (15) or activate it to thrombin (19), depending upon time and concentration factors. Traces of thrombin can apparently change the plasma type A α -globulin to the active serum type and in higher concentrations it can destroy A α -globulin (23). It reacts with antithrombin (25), and it has been claimed that thrombin has an effect on the stability of organic and inorganic colloids (10). Quick suggested that thrombin labilizes platelets (16). Recent data also indicate that it may have hyper-tensinase activity (2). It is conceivable that all of these actions are mediated through one specific portion of the thrombin molecule. On the other hand, it may be that different parts of the molecule are responsible for the permutations.

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Central Mechanisms for Recovery of Neuromuscular Function

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In treatment of paralysis of various types by means of therapeutic exercise, the role of the muscle is usually considered predominant while the important role of the central nervous system in recovery of voluntary motor function is often overlooked. This point of view is evident in two recent reviews (7, 9).

Physiologists have long known that after lesions of the central nervous system, functional recovery may occur to a considerable extent despite permanent anatomical destruction of an essential motor pathway, such as the corticospinal tract. This recovery is believed to occur through a process of compensation by which other intact pathways, largely extrapyramidal, substitute for or take over the function of the damaged tract (8). The process of compensation is considered to occur spontaneously. From our study of several thousand patients, with various types of paralysis, undergoing intensive neuromuscular rehabilitation, it has been possible to demonstrate that substitute pathways for voluntary motion can be developed rapidly and effectively through training. New techniques applied in neuromuscular reeducation have shed light on the predominant role of central neural mechanisms in recovery of motor function (5, 6). The use of these techniques has also clarified the fundamental functions of the major motor mechanisms in the central nervous system.

A basic component of the technique of muscle reeducation in paralysis is maximal effort by the patient to contract paralyzed muscles against the resistance of the therapist. It was found, however, that the response of the paralyzed muscles could be further increased to a marked degree by a number of mechanisms which facilitate central stimulation and apparently produce summation of subliminal impulses in the motor centers. These mechanisms are:

(1) *Mass movement patterns* (5). These include additive movements and other complex patterns which are still available for central stimulation after almost complete destruction of both corticospinal tracts. These patterns presumably function through extrapyramidal mechanisms. An example of such a pattern in the lower extremity is: flexion, adduction, external rotation of the hip; flexion of the knee; anterior tibial and extension of the toes. Even though the anterior tibial is completely paralyzed from a corticospinal lesion, it will contract and dorsiflex the ankle as a result of overflow of energy through the total central pattern from the better innervated hip and knee muscles contracting maximally against resistance.

Gellhorn (8) showed that a similar specific functional association can be demonstrated in the monkey between the triceps and the flexor carpi, between the hamstrings

and anterior tibial, etc. When the motor cortex to the hamstrings is stimulated, the anterior tibial is also facilitated. The motor points for synergic muscles are adjacent on the surface of the cortex.

It is our contention that the basic voluntary motions are specific mass movement patterns, resulting in simultaneous synergic movement throughout the limb from its proximal to its distal end, with specific synergic components of the trunk musculature also entering into the total pattern. Isolated motions are apparently derived from these fundamental patterns. These same mass movement patterns are evident in normal activity, i.e., in heavy work and sports such as chopping wood, kicking a football, pitching a baseball, etc. In addition to facilitation of voluntary motion, maximal stimulation of these patterns results in marked and prolonged relaxation of spasticity, muscle spasm, and Parkinsonian rigidity.

(2) *Reflexes* (5). Placing a paralyzed muscle under greater tension by stretching will often increase the response in voluntary contraction of that muscle. In hemiplegia, the voluntary contraction of the paralyzed hamstrings for knee flexion is markedly facilitated in the sitting position when these muscles are placed on stretch. Stretching the anterior tibial will also increase the response of the whole lower extremity mass movement pattern, not only in the ankle, but in the knee, hip, and lower trunk muscles as well. Gellhorn (8) found a similar phenomenon on cortical stimulation in monkeys. Stretch of the triceps greatly increases the response of that muscle to stimulation of the motor cortex. Also stretch of the extensor carpi increases the response to cortical stimulation, not only of that muscle but of the synergistic biceps as well. Furthermore, he showed that subthreshold stimulation can produce a response in a stretched muscle by summation of the weak cortical with stronger proprioceptive impulses.

Other reflexes can also summate with voluntary effort to produce contraction in paralyzed muscles, i.e., the mass flexion reflex of the lower extremity (von Bechterew), tonic neck reflex of Magnus, gag reflex, etc.

(3) *Quick reversal of antagonists*. In this technique, the antagonistic motion is carried out against maximal resistance, and then suddenly the agonist motion is performed as rapidly and strongly as possible. This method of facilitation summates with mass movement patterns for still greater response. Quick reversal of antagonists is evident in normal activity in chopping wood, the boxer's punch, the golf swing, the farmer using the scythe, the football kick, etc. The fundamental mechanism involved was demonstrated years ago by Sherrington and termed "successive induction," i.e., stimulation of the flexion reflex strongly facilitates an antagonistic extension reflex in the same limb immediately afterward.

The technique of quick reversal has been especially useful in patients with lesions affecting function of the cerebellar hemispheres.

(4) *Rhythmic stabilization* (6). In this technique, the patient attempts to hold a rigid position and the therapist alternately and rhythmically applies maximal resistance in an attempt to move the limb. As an ex-

ample, the patient holds the wrist rigid in the neutral position and the therapist alternately and rhythmically applies resistance to the wrist extensors, then the wrist flexors, then extensors, and so on. The patient is alternating isometric contractions and, as the procedure is continued, the power of the isometrically contracting muscles increases. This technique is applied in mass movement patterns and the resulting summation of central stimulation greatly increases the voluntary contraction of paralyzed muscles. There is usually an after-discharge following rhythmic stabilization. This technique not only markedly facilitates voluntary contraction of paralyzed muscles but also inhibits spasticity, muscle spasm, or rigidity.

This method also utilizes the principle of successive induction but with alternating isometric contraction of the muscles. Patients with many different types of paralysis including lesions of the corticospinal tracts, of basal ganglia (including Parkinson's disease and athetosis), and of lower motor neurons responded remarkably to this technique. On the other hand, patients with even a slight degree of cerebellar involvement failed to carry out rhythmic stabilization successfully. In fact, application of rhythmic stabilization was a very sensitive test of cerebellar function. It became apparent that the cerebellum is probably an essential part of the central mechanism for rhythmic stabilization.

A careful study of a large number of patients, with cerebellar disease from multiple sclerosis or familial cerebellar ataxia, revealed that the disability correlated closely with the deficiency in carrying out rhythmic stabilization but failed to correlate at all with the degree of paralysis of the muscles. It soon became apparent that the intention tremor, hypotonia, dysmetria, rebound, and marked fatigability in these cases was related fundamentally to a deficiency in power, range, and particularly endurance of isometric voluntary contraction of individual muscles. The inability to perform rhythmic stabilization by alternating isometric contraction of antagonists was related to a more basic deficiency of isometric contraction of each antagonist individually. Isotonic voluntary contraction was involved much less, if at all. This hypothesis has been tested by treating patients with the cerebellar syndrome by developing the power, range, and duration of isometric contraction of the affected muscles. For this purpose, the quick reversal technique combined with mass movement patterns against maximal resistance was particularly effective. It has been possible to demonstrate that this procedure im-

proves and may in some cases completely eliminate the cerebellar syndrome and its attendant disability. As the deficiency in isometric contraction improved, the ability to perform rhythmic stabilization also improved, and the whole syndrome of asynergia, including intention tremor, dysmetria, rebound, hypotonia, and fatigability was benefited concomitantly. This is the first effective therapy developed for the cerebellar syndrome and also the first time that isometric contraction was implicated as the basic deficiency involved (1, 2, 4). From these observations, it is reasonable to set up the hypothesis that the fundamental function of the cerebellar hemispheres is to facilitate voluntary isometric muscular contraction.

The function of the corticospinal mechanism appears to be stimulation of voluntary motion and inhibition of spasticity. Spasticity is dependent on the stretch reflex and facilitating postural mechanisms in the reticular formation and vestibular nuclei (5). An analysis of Parkinson's disease suggests the possibility that the area in the basal ganglia, damage to which produces this syndrome (substantia nigra and globus pallidus), has the fundamental function of facilitating isotonic voluntary muscular contraction. There is evidence that the cerebellum has the opposite effect of facilitating voluntary isometric contraction. The deficiency in Parkinson's disease, aside from the tremor, appears to be related to weakness, slowness, fatigability, lack of range, and difficulty in initiating isotonic voluntary motion. Isometric contraction is carried out much more strongly and rhythmic stabilization is performed effectively. Improvement in voluntary isotonic contraction through application of mass movement patterns and rhythmic stabilization has significantly improved the disability, accompanied by striking improvement of rigidity.

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Comments and Communications

A Collaborative Genetical Survey of the Human Populations of the Pacific Area

Roy T. Simmons, of the Commonwealth Serum Laboratories, Parkville, N.2., Victoria, Australia, and the members of the Department of Anthropology and Sociology of the University of California at Los Angeles have undertaken a long term collaborative project for the genetical survey of the native populations of the Pacific area. Mr. Simmons has been appointed a Research Associate in the Department. Formerly it was possible to conduct complete serological surveys in the field, using simple procedures, but all of the desirable genetical data can no longer be collected in this fashion. In the last few years the complicated developments of the important Rh factors, which require laboratory facilities for proper testing, together with the rarity of proper sera for the identification of the more recently isolated alleles at this and other loci, require that blood samples be transported from the field to the laboratory for efficient genetical testing.

Mr. Simmons has demonstrated the feasibility of collecting blood samples from any part of the world where local refrigeration can be obtained until the iced thermos flasks reach the feeder line of a main trunk air route. Trial samples have been successfully flown from London to Melbourne with no loss. Since 1944, Simmons and his collaborators, among whom John J. Graydon is the most active, have tested and published genetical analyses based upon blood samples flown to their laboratory from aboriginal groups in Australia, Fiji, New Caledonia and the nearby islands, the Admiralty Islands, the southern coastal region of New Guinea, Leyte in the Philippines, and New Zealand. Additional data have been published from various populations transiently accessible in Australia during the recent war: Japanese, Hollanders, and various Indonesian groups. Samples recently have been tested from the Gilbert Islands, from five populations in Borneo, and from two provinces in China, and the results will be published within the near future. Arrangements have been made for the collection of blood and saliva samples from the inaccessible Mt. Hagen region of New Guinea. These data represent a substantial addition to the knowledge of the distributions of the more recently discovered serological genes in human populations. This impressive achievement is the more remarkable in that much of it was accomplished during the pressure of the war years by a team working outside of their primary obligations as research biochemists. It is a fitting tribute to Simmons and Graydon that today our knowledge of the population genetics of Australasia is more advanced than in other regions of the world. They will continue their own work of this type in the future, in addition to collaborating in the present program.

Collaboration between the Australian and American institutions arose from the feeling that the unusual contributions made by Simmons and his team under difficult circumstances might be further facilitated with organizational, financial, and analytical assistance. Our arrangement is such that all samples will be flown via air freight to Simmons in Melbourne for the genetical testing. The Department of Anthropology and Sociology at the University of California at Los Angeles will assist in establishing the most critical areas for survey in terms of a comprehensive anthropological survey, in arranging field contacts with personnel in such regions for the collection of samples, and in enlisting financial support to defray the cost of air freight and the various necessary equipment items. Primary data will be published by Simmons and his laboratory and field collaborators. Periodically, members of both institutions will collaborate in publishing analytical syntheses of all the available data.

The task of the joint program is visualized in terms of identifying those populations critical in providing essential genetical data relative to problems centering on the dynamic processes of human evolution. Various aspects include the identification of the basic racial populations in the Pacific area, evaluation of the degree of hybridization in tested populations, the estimation of rates of gene flow, the exploration of the process of genetic drift, and ultimately the design of experiments to attempt to determine the influence of natural selection upon serological antigens. Preliminary phases will require a random sampling of populations on a broad areal basis to establish the major genetical clines in the region. Later investigations will include detail sampling in terms of groups which approximate effective breeding populations. Research emphasis will change as the basic genetical data increase.

At the present time Simmons can identify allelic serological genes at four loci on different chromosomes:

1. The O-A-B locus; Anti-O, -A₁, -A₂, -B sera are now available.
2. The M-N locus; Anti-M₁, -M₂, -N and -S.
3. The Rh locus or loci; Anti-C, -C⁺, -D, -D⁺, -E, -e, -e sera.
4. The Lewis locus; Anti-L₁ now available, and Anti-L₂ anticipated in the near future.

The inclusion of saliva samples allows the identification of the two alleles known at the secretor locus, which seem related in a way as yet undefined to the Lewis antigens. PTC taste-testing will be conducted where feasible. As additional sera for the above loci, or others, become available to Simmons, tests will be extended to include those routinely.

This project has received a grant from the Research Committee of the University of California at Los Angeles to allow the initiation of field work. We are indebted

to the Viking Fund for a generous sustaining grant to cover field expenditures of the survey during the years 1950-1952. The Pacific Science Board has approved the project and has agreed to arrange for naval air transport where feasible throughout the Trust Territory. Harold Coolidge, executive secretary of the Pacific Science Board, now on several months' tour of the Trust Territory, is generously arranging for field personnel to assist in the collection of blood and saliva samples. The present plan is to concentrate survey efforts in Micronesia and Polynesia, since naval air transport will not be available there later than July 1, 1951. During this period it is planned to obtain samples from the ports of call of the floating laboratory, *U.S.S. Whidbey*. These latter results may be of special importance, since detailed physiological and medical data will be routinely obtained by medical personnel from the same individuals.

This survey promises an efficient and inexpensive way in which our knowledge of population genetics can be rapidly expanded and by means of which evolutionary hypotheses can be tested. Individuals who may be able to assist in the establishment of proper field contacts in the Trust Territory at this time, or in Australasia or Southeastern Asia at a later date, are asked to communicate with the undersigned.

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The Concept of "Internal Compensation"

The purpose of this communication is to propose an experimental solution to the problem of the meaning of "internal compensation" as applied to meso compounds. A few years ago C. R. Noller (*Science*, 1945, 102, 508) pointed out the lack of meaning of the phrase "inactive by internal compensation." His argument was based on the well-known thesis (founded on a mass of empirically correlated data [e.g., cf. H. Gilman, *Organic chemistry*, 2nd Ed. New York: John Wiley and Sons, 1943. P. 214 ff.] and on theoretical grounds [e.g., cf. W. J. Kauzman, J. E. Walter, and H. Eyring, *Chem. Rev.*, 1940, 26, 339]) that the symmetry properties of a molecule as a whole determine the nature of its interaction with polarized light. The epitome of this argument is that symmetric molecules do not, and dissymmetric molecules do, have optical rotatory power. A symmetric molecule may be differentiated from a dissymmetric one by the criterion that the former is, whereas the latter is not, superimposable on its mirror image (enantiomorph). In the light of this concept, Noller showed that some staggered configurations of meso compounds are dissymmetric and hence should be optically active (cf. also G. W. Wheland, *Advanced Organic Chemistry*, 2nd Ed. New York: John Wiley and Sons, 1949. P. 191). In support of his argument, Noller proposed the synthesis of some substituted succinic acids which should be stabilized in the staggered configuration, due to the bulkiness of the substituents.

In a rebuttal to C. R. Noller's paper, George F. Wright (*Science*, 1946, 104, 190) has taken the position that a

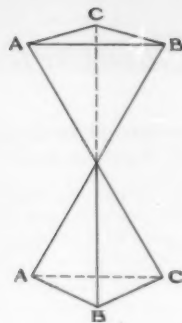


FIG. 1.

meso compound may be regarded as made up of two nonsuperimposable enantiomorphous halves, each rotating plane-polarized light in opposite directions to the same extent. This argument is taken as a justification of the term "internal compensation," although the symmetry property of the molecule as a whole (i.e., the criterion for optical rotatory power as stated here) is apparently disregarded. Arguing on the basis of this misconception, Wright has predicted that the staggered form (Fig. 1) should be optically inactive, whereas the symmetry criterion (already mentioned) shows that this molecule must be optically active.

In view of this controversy, the isolation and resolution of such staggered meso forms would constitute an important experimental contribution. The use of Noller's compounds, however, meets with two serious objections: (1) The compounds have iodine (or iodine and bromine) atoms on adjacent carbon atoms; they would therefore be expected to be quite unstable and it is doubtful that they could be synthesized. (2) The as-

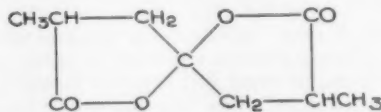


FIG. 2.

sumption that rotation about the central carbon-carbon bond is restricted, based essentially on the study of molecular models, leaves the activation energy corresponding to the rotational energy barrier an unknown quantity; hence the rate of racemization might still be appreciable at room temperature, necessitating special techniques and complicating the task of separation and resolution.

The experimental solution of the problem may be found in the resolution of compounds having the spiran ring system, as exemplified by 3,8-dimethyl-1,6-dioxaspiro[4.4]nonane-2,7-dione (Fig. 2). Such compounds, in which the ring systems are at right angles to each other, and which in addition contain two dissymmetric groupings, can exist in six stereoisomeric forms (cf. Wheland, G. W., *loc. cit.* p. 201). Two of these stereoisomers, which are enantiomorphous (Figs. 3 and 4), have the



FIG. 3.



FIG. 4.

following crucial properties, shared by the compound shown in Fig. 1: (1) they are dissymmetric molecules; (2) they may be regarded as made up of two nonsuperimposable enantiomorphous halves. The synthesis and isolation of the DL-pair shown in Figs. 3 and 4 can be accomplished readily and in good yield (Sutter, H. and Wijkman, N., *Ann. Chem.*, 1935, 519, 97); the partial resolution of such a DL mixture might be accomplished by chromatography on a lactose column. Since ring systems usually exhibit high specific rotations (Kauzman, W. J. and Eyring, H., *J. chem. Phys.*, 1941, 9, 41), partial resolution should be readily observable.

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Zoological Nomenclature: A Reply

A series of replies to our statement on "Basic Issues in the Controversy on Zoological Nomenclature" (*Science*, 1949, 110, 594), has been published (*Science*, 1950, 111, 234). Having placed our position on record, and realizing that continued discussions may only increase animosity, we should prefer to rest the case, and may do so henceforth. However, statements from such distinguished zoologists command our attention and merit the courtesy of acknowledgment. It is also our duty, particularly as we are charged with misstatements of fact, to reexamine our position carefully in the light of the comments, to admit any errors, and to reaffirm our beliefs if we are still convinced that they are sound.

The second paragraph of our statement was a summary of the Paris meeting, and not in itself an argument or criticism. It was intended to state this as accurately as possible in a brief and generalized way, although brevity unfortunately precluded qualifying phrases and details. As the factual basis of our position, it is important to analyze that paragraph for error. In order to compare seriatim our summary with the replies, the whole paragraph can be divided into eight points:

(a) Composition of the commission at Paris: Not questioned in any of the six letters.

It is regrettable that offense was taken, and "invidious innuendo" charged, at our use of the term "regular commissioners." We desired only to distinguish clearly between the temporary alternate commissioners, appointed for the duration of a congress, and the 18 regularly elected members of the International Commission. Although the latter is sometimes referred to as the "Permanent Commission," we do not feel that Mr. Francis Hemming's expression "permanent members" is entirely appropriate, for that connotes permanent tenure, whereas the members are elected for specified terms, or to complete unexpired terms.

(b) "Program prepared at great length by the Secretary": Not challenged in the replies.

(c) "Authorization for a rewriting of the code around a set of stated principles" (italics ours, here and elsewhere in this paper): The italicized portion was denied by Edward Hindle and N. D. Riley, who maintained that the commission "took precise decisions as to . . . amendments." Henning Lemeche and Ragnar Spärck, however, accepted our phraseology. Secretary Hemming, who said that our statement contained "misstatements on questions of fact," did not mention this point.

Probably our wording was too abbreviated, and a more explanatory presentation is desirable. At Paris there was authorized an extensive rewriting of the code, to be based on numerous amendments involving almost every article, and including changes in the wording, changes of some principles, introduction of some new material, and review of all opinions with the purpose of accepting or rejecting any pertinent principles for use in the revised code. In most cases, decisions were only "in principle," i.e., exact and final wordings were not adopted but it was only decided that "words should be inserted to make it clear that . . ."

In passing, we note the assertions that "The principles of the rules remained unaltered" (Hindle and Riley) and "Such change of the rules was out of the question" (Lodovico di Caporiaceo). It appears to us that such changes as those from binary to binominal nomenclature and from rule of the first reviser to page precedence, represents basic alterations in fundamental principles of the code. Incidentally, in view of their lament against purely nomenclatural changes, the Chicago Discussion Group will no doubt be interested to learn that the above changes were made retroactive.

(d) "Action on a large number of individual amendments, none of which had previously been voted on by the commission": The accuracy of this statement was not questioned. Hindle and Riley commented that to adopt the course suggested by us would entail a double vote on each proposal, "a most curious and novel idea." On the contrary, we note that this has been standard procedure in the International Commission. The by-laws of the commission, Article IV, Section 1, provide that the commission's report to the congress shall include "(a) recommendations involving any alteration in

the Règles . . . but no such recommendation is to be reported, unless it has received:—(i) a majority vote of the whole Commission, that is to say ten (10) votes, and (ii) the unanimous vote of all Commissioners present at the meeting” (by-laws, revised 1939; essentially the same wording in the original by-laws adopted in 1910, cf. *Verh. VIII Int. zool. Kongr.*, Graz, 1910, 321.) At various times, the procedure has been published in some detail (e.g., Stiles, C. W. *Zool. Anz.*, 1912, 41, 37; and *Ent. News*, 1929, 40, 329; Bolívar, C. *Conf. Reseñas cient. Soc. Españ. Hist. nat.*, 1929, 4, 165). Such articles served notice in advance of a congress of proposals for changes in the code, and zoologists were invited to send comments to any member of the commission.

The Commission sits, usually for one week, immediately prior to the meeting of the Congress. According to procedure, no proposition is considered unless it is submitted to the Commission at least one year prior to the meeting of the Congress. A preliminary informal vote on propositions precedes the meeting of the Commission, and no proposition is subject to discussion unless it receives a majority vote in this preliminary consideration. No proposition is reported to the Congress unless it receives the unanimous vote of the entire Commission present at the meeting.

(Stiles, C. W. *Ent. News*, 1929, 40, 332.)

(e) “Entrusting to ‘jurists’ the preparation of the text of the new code in ‘watertight legal language’”: Not challenged by Hemming or Caporiacco, and Hindle and Riley stated that “We certainly did decide . . . we would leave it to . . . a jurist [a jurist?] to translate these decisions and amendments into formal language.” Other comments called attention to the “special drafting committee” (Lemche and Spärek) or “editorial committee” (J. Chester Bradley).

Our statement was essentially a briefer wording of Secretary Hemming’s press release (*Science*, 1948, 108, 156): “Recognizing the highly technical problems inevitably involved in the preparation of a substantive text . . . [it was decided] that this task shall be entrusted to jurists. . . .” Further, the press release stated that “The text prepared by the jurists will be submitted to the members of the Commission for final approval, and the task of considering any drafting points which may emerge from the foregoing consultation has been entrusted to a special committee of three. . . .” We omitted mention of these arrangements because Secretary Hemming had stoutly maintained in correspondence (e.g., Hemming to R. C. Moore, *J. Paleontol.*, 1949, 23, 228) that the *only* object of the review is to ensure that the jurists’ text corresponds precisely to the decisions reached at Paris. In other words, it appears that this “final approval” by the commission can be little more than proofreading.

(f) “A complete reorganization of the commission . . .”: Not challenged by anyone.

(g) “Most of this was without advance notice to zoologists or to the commission”: Challenged by Hemming, Lemche and Spärek, Hindle and Riley, Caporiacco, Bradley. Considerable difference of opinion on meaning is involved here. It is a vital point to clarify.

Regarding advance notice to the regular commissioners, we cannot of course speak for them, but we had sufficient assurances to amply justify the point as we stated it. Can the secretary show that the commissioners knew and approved of the plans for rewriting the code? Did they approve the report on binary nomenclature? On the change from first reviser rule to page precedence? The report on secondary homonyms and that on infraspecific categories? Were these and other proposals before the commissioners for one year before Paris? Was there a preliminary vote by the entire commission on these and other amendments? Statements by the commissioners in the far-flung parts of the world, by Cabrera, do Amaral, Esaki, Pearson, Richter, etc., as well as from those closer home, as to whether these proposals and plans had been placed before them by the secretary well in advance of Paris and that the Paris revision was carried out with their full knowledge and consent, would be the only convincing answers.

It is pertinent here to note that the last twenty lines of Secretary Hemming’s second point are based on his statement that we contend that “matters of importance should be considered by the commission *only* by correspondence conducted between members of the commission during intercongress periods,” etc. We have searched our statement in vain for such a proposed limitation. We support the procedures prescribed in the by-laws of the commission: preliminary consideration by the commission for at least one year (Art. III, Sec. 2), a preliminary vote, and finally the formal vote (Art. IV, Sec. 1).

As for advance notice to zoologists in general, we agree that for amendments and rewriting of the code, there is no technical requirement for preliminary announcement. However, we point to the fact that it was commission practice to give such notice (cf. point d). Further, such a procedure seems eminently reasonable and desirable to keep zoologists informed and to allow opportunity for expression of views on pending proposals. We may perhaps suggest that such notice ought to be a requirement, in addition to the provisions already in the by-laws of the commission, and that all such provisions be faithfully observed.

Mr. Hemming visited the U. S. and Canada briefly in December 1947. From records kept during the meetings, it is estimated that slightly over 300 different individuals heard his discussions, though relatively few actually voiced opinions of their own. Rather detailed notes were taken by one of us during the meetings at the Smithsonian Institution and elsewhere. One of the main subjects emphasized by Secretary Hemming was a “new edition of the code,” with English and French versions on opposite pages, a history of the development of the code, and quotations from opinions arranged under the articles to which they refer. This was expected to be a volume of 300 pages, and to cost about two dollars, thanks to a subsidy from the Royal Society of London. Because of the considerable work that had already been done on it, the edition, it was said, could probably be issued within two or three months after the Paris meet-

ing. The report on this project was greeted with enthusiastic approval. Further, not only was it discussed by Mr. Hemming at open meetings, but he specifically outlined it at a conference with the secretary of the Biological Society of Washington, which deferred plans to reprint their well-known edition of the code because of the commission's project.

At no time in the hearing of any one of our group—and some member or members attended all meetings on Mr. Hemming's itinerary except Princeton and New York, where about a dozen zoologists in all were present—did he discuss rewriting of the code as carried out at Paris.

It has been made to appear that prior to the Paris meeting we were fully cognizant of the plans through personal contacts with Secretary Hemming, and even that we "freely endorsed those plans." This we deny. To maintain that we were "fully informed" and that we "freely endorsed" the plans is a truly profound misunderstanding.

It may also be pointed out that there was no reason for believing that the code was to be revised, for earlier published statements by the president and the secretary of the commission were in line with the plans for a new edition as outlined in the secretary's discussions in America:

(Jordon, K. *J. N. Y. Entomol. Soc.*, 1944, 52, 385.) "The International Commission have made arrangements to publish as soon as possible . . . an up-to-date edition of the International Code of Zoological Nomenclature."

(Hemming, Francis. *Bull. zool. Nomenclature* 1 [pt. 6], 1945, lvi-lvii.) He discusses a "forthcoming edition" of the International Code, with French and English versions on opposite pages. "During the year 1944, a substantial start was made with the preparation of the new edition of the Règles Internationales. . . ."

(h) "Neither the Commissioners nor the alternates at Paris had opportunity to study the voluminous agenda in advance of the meeting": Not challenged. We note that Secretary Hemming states that

. . . It will be obvious to anyone possessing any experience in the conduct of discussions regarding intrinsically difficult questions that the chance of obtaining a solution by correspondence is extremely remote, the only satisfactory procedure . . . being full and free discussion round the table.

We submit that it will also be obvious that success of such round table discussions depends greatly on a foundation of correspondence, study, and preparation by all participants. For "intrinsically difficult questions" we cannot highly regard the chances of obtaining satisfactory solutions by discussions on crowded agenda during long and weary hours without opportunity for preliminary consideration, study of the evidence, and conference with colleagues of diverse backgrounds and viewpoints, followed by calm reflection. With the appearance of the three volumes and 1,000 pages of agenda and minutes, zoologists can see for themselves the mass of material in such a short time at Paris.

In view of Secretary Hemming's lack of regard for the worth of correspondence, it is interesting to note in the

last paragraph of his letter that the Paris congress hoped that "reasoned statements" of views on amending or expanding the decisions taken at Paris would be submitted

. . . to the commission, so that those views might be considered during the present intercongress period with a view to the formulation by the commission of suitable proposals for consideration by the Copenhagen congress in 1953.

Is it unreasonable to maintain that this procedure could and should have been in operation before Paris also, and thus have been consistent with the by-laws, and with the traditional procedures of the commission? Is it not a better principle of construction to shape and fit the foundation stones with painstaking care, than to make haste to erect an imposing edifice with proviso for reworking the foundation as soon as the building is completed? Zoologists will recall that the present code was adopted only after a considerable period of study and submission to several congresses; ten years (1895-1905) elapsed between initiation of the project and final publication of the code.

Having now compared our summary of the facts with the comments, and believing that in no essential particular were we shown to be in error, we reaffirm the position taken in our previous statement.

It may be permissible at this time to comment on two charges that reflect on the Washington Group. (1) It was stated that in effect we condemn the commission and its secretary "unheard." On the contrary, we have had extensive correspondence with the secretary on the subject of the Paris meetings. If "unheard" is meant in the sense of publication, we may call attention to Secretary Hemming's press release after the Paris meeting, which has appeared to our knowledge in seven journals in five countries and three languages, including *Science* for Aug. 13, 1948; to Dos Passos in *Lepidopterists' News*, Feb. 1949; to Riley in *Lepidopterists' News*, June 1949; and to Hemming's letter to R. C. Moore, *J. Paleontol.*, March 1949. For our part, no comments about the Paris meeting had been published by the Washington Group until that in *Science* for Dec. 2, 1949.

(2) Mr. Hemming stated that we have "severely criticized" the introduction of certain changes into the code, and he later referred to our stand as being "tantamount to an uncompromising opposition to all reforms of every sort." However, it should be clear to everyone that we do not oppose reform *per se*, as can easily be shown from the published record (cf. *Science*, 1947, 106, 315; *Science*, 1948, 107, 166; *Science*, 1948, 108, 37). These publications demonstrate our awareness that portions of the code need study with a view to clarification or amplification. What we do question, and oppose, is the failure to utilize the normal procedures of consideration by the regular commission, and the haste with which decisions were made at Paris.

Our belief in the necessity and value of ample time for study and deliberation, and in the desirability of giving other zoologists full opportunity to express their views on definite proposals, is well illustrated by the procedure

followed by our group in studying Articles 19 and 20 of the code.¹ We cannot help but feel that similar consideration for the zoologists of the world would have greatly promoted confidence and support for international nomenclature.

It is quite true—and it may be made clear at this time—that we dislike some actions that were taken at Paris. Some we regard as catastrophic. On the other hand, some we approve. But all that is not our present concern. Had all these matters, and the over-all rewriting of the code, been considered in line with the established and customary procedures of the International Commission, we should have felt, as we have in the past, morally obligated to follow the decisions even though in many cases they were not to our liking.

In conclusion, it should be emphasized that in our statement we were not primarily concerned with *what* was done, but *how*. Accordingly, we do not propose to reply here to the letter from Karl P. Schmidt and part of that from Lemche and Spärek, which are not strictly relevant to the present controversy. Even assuming that their views are shared by the majority of zoologists, of which we may be permitted to have a reasonable doubt,² our primary concern is not with philosophy but with procedure. If, as the Chicago Group believes, the commission should, even more than in the past, “resort to the sword of fiat decision,” certainly it would then become even more imperative that established procedures and protocol be meticulously observed, and that the commission so conduct its affairs that all zoologists would have confidence that decisions had been carefully arrived at after due deliberation and with full consideration for all points of view and all pertinent evidence.

¹ An interesting meeting of April 14, 1947 on “The status of errors and emendations” led to the appointment of a study committee. After many hours of work, a report was drafted, distributed to the entire group, and discussed at meetings of May 12 and June 9, 1947. The final version was approved, and published (*Science*, 1947, **106**, 315) in order to get any suggestions from other zoologists before sending it to the commission. After receiving numerous comments, the committee reexamined the problem and revised the statement in a number of particulars. The revised proposal was published (*Science*, 1948, **106**, 37), with a note that it was being presented to the commission as a formal proposal “in order that the Commission might begin consideration of it at the Paris Congress.” (We understood, of course, that because of the one-year rule, we could expect only preliminary consideration at Paris.) Beyond this step, we anticipated that our proposal would eventually be published in the *Bulletin of Zoological Nomenclature*, that any zoologist could comment in those pages or in correspondence to the commission, and that the commission of 18 members would then consider the proposal and all comments, take a preliminary vote, and finally, at the next international congress, recommend any appropriate changes in the code.

² Of the replies received to our “Proposed Petition on the Use of the Plenary Powers” (*Science*, 1948, **107**, 543), 156 supported the petition, 7 would go farther in restricting suspensions of the rules, and only 5 opposed the petition and expressed themselves as strongly in favor of suspensions. Incidentally, this petition with all signatures and comments was sent to Mr. Hemming “for presentation and discussion at the meeting of the Commission in Paris,” but we are informed that it was neither presented nor mentioned.

In the long view, no problem in zoological nomenclature is ever so urgent that confidence in the commission need be sacrificed solely to produce an immediate decision.

STEERING COMMITTEE
NOMENCLATURE DISCUSSION GROUP

Washington, D. C.

Scientific Book Register

- Antibiotics: A Survey of Penicillin, Streptomycin, and Other Antimicrobial Substances from Fungi, Actinomycetes, Bacteria, and Plants.** Vols. I and II. H. W. Florey et al. New York-London: Oxford Univ. Press, 1949. 1774 pp. \$29.75 the set.
- Sterkfontein Ape-Man Plesianthropus.** Robert Broom, J. T. Robinson and G. W. H. Schepers. Pretoria, South Africa: Transvaal Museum, 1950. 117 pp.
- Volcanological Observations.** Frank Alvord Perret. Washington, D. C.: Carnegie Institution of Washington, 1950. 162 pp. \$5.00 paper; \$5.50 cloth.
- Analytical Absorption Spectroscopy: Absorptimetry and Colorimetry.** M. G. Mellon, Ed. New York: John Wiley; London: Chapman & Hall, 1950. 618 pp. \$9.00.
- Centennial.** Collected papers presented at the Centennial Celebration, Washington, D. C., September 13-17, 1948. Washington 5, D. C.: American Association for the Advancement of Science, 1950. 313 pp. \$5.00.
- An Introduction to Modern Genetics.** 2nd printing. C. H. Waddington. London: Allen & Unwin; New York: Macmillan, 1950. 144 pp. \$3.25.
- Chemical Index of Minerals.** Max H. Hey. London, England: British Museum (Natural History), 1950. 609 pp. £1 10s.
- The Practice of Medicine.** 5th ed. Jonathan Campbell Meakins. St. Louis, Mo.: C. V. Mosby Company, 1950. 1558 pages. \$13.50.
- Advanced Plane Geometry.** C. Zwikker. Amsterdam: North-Holland Publ.; New York: Interscience, 1950. 299 pp. \$6.00.
- Textbook of Endocrinology.** Robert H. Williams, Ed. Philadelphia: W. B. Saunders, 1950. 793 pp. \$10.00.
- The Natural Philosophy of Plant Form.** Agnes Arber. London-New York: Cambridge Univ. Press, 1950. 247 pp. \$5.00.
- Mechanics of Deformable Bodies.** Lectures on Theoretical Physics, Vol. II. Arnold Sommerfeld; trans. from 2nd German ed. by G. Kuerti. New York: Academic Press, 1950. 396 pp. \$6.60.
- Introduction to the Bacteria.** C. E. Clifton. New York: McGraw-Hill, 1950. 528 pp. \$5.00.
- Chemistry and Industry of Starch.** Rev. 2nd ed. Ralph W. Kerr, Ed. New York: Academic Press, 1950. 719 pp. \$11.50.
- General Biology for Colleges.** 2nd ed. Gairdner B. Moment. New York: Appleton-Century-Crofts, 1950. 680 pp. \$5.00.

News and Notes

George H. M. Lawrence, taxonomist of the Bailey Hortorium at Cornell University, is on leave for a year of study at European botanical centers. Prof. Lawrence will work from August until January at the British Museum of Natural History, where he will make a study of the Philip Miller botanical collection.

Francis Michael Forster has been appointed director of the Department of Neurology, Georgetown University School of Medicine. He was formerly associate professor of neurology at Jefferson Medical College.

Gordon R. Kamman, associate clinical professor of psychiatry, University of Minnesota, has been appointed deputy commissioner of mental health for the State of Minnesota.

Giulio L. Cantoni, assistant professor of pharmacology at Long Island College of Medicine, has been appointed associate professor in the Department of Pharmacology of Western Reserve University School of Medicine.

Peter R. Morrison, zoologist and physiologist, University of Wisconsin, will head a group of scientists from the university in a two-month study this summer of temperature regulation and metabolism of Alaskan animals. This research, under contract to the U. S. Air Force, will be carried out at the Arctic Aero-medical Laboratory, Ladd Air Force Base, Fairbanks.

Joseph W. Alsop, senior member of the Board of Control of the Connecticut Agricultural Experiment Station, has resigned his post after 40 years of continuous service. He is succeeded by **Edmund W. Sinnott**, director of the Sheffield Scientific School of Yale University.

Mark H. Lepper has been appointed to the University of Illinois College of Medicine as an associate professor in the Department of Pre-

ventive Medicine. Dr. Lepper has done research work on the therapies of infectious diseases and toxic reactions to drugs—particularly drug allergy and inhibition of antigen-antibody reactions.

Conrad B. Rivera, of the Philippine Army Medical Corps, has been assigned to the Department of Physical Medicine and Rehabilitation at the University of Illinois College of Medicine for training that will enable him to set up a similar department in a general hospital of the Philippine Army. Prior to his present assignment, Dr. Rivera spent four months in training at the Percy Jones General Hospital, Battle Creek, Michigan.

Visitors to U. S.

Koichi Aki, director of the Resources Council Secretariat, and **Sikazo Inaura**, engineer-in-chief of the Ministry of Construction, both from Tokyo, are in the U. S. for about three months. They visited the U. S. Geological Survey last month to study water resources management and they hope to cover most of the major water development projects in this country during their stay.

Helen Petralia, chief nurse of the Ministry of Hygiene in Greece, is in the U. S. for four months, studying public health programs and the organization and administration of schools of nursing.

Recent visitors at the Communicable Disease Center, U. S. Public Health Service, Atlanta, Georgia, were **Juan C. Gomez**, chief of the Laboratory of Engineering, **Ezequiel Sutil**, inspector-supervisor of malaria, Service Section of Medical Activities, **Octavio M. Suarez**, inspector-supervisor of malaria, **Roberto Escalona**, inspector of malaria, and **José Antonio Lopez**, chief of service, Medical Activities Section, all of the Division of Malariology of the Venezuelan Ministry of Health and Social Assistance; **Carlos A. Alva-**

rado, consultant on malaria, Pan American Sanitary Bureau, Tucumán, Argentina; **Eyvind Ek**, assistant director, Bureau of Maternal and Child Health, Oslo, Norway; **Albert Herrlich**, professor of internal and tropical medicine, University of Munich; **Deogracias P. Caro**, Public Health Laboratory, Manila Health Department, Manila; **Hsi-Chow Chen**, Malaria Institute, Chau Chow Malaria Laboratory, Kao Hsiung, Formosa; and **M. Hassan Morshed**, World Health Organization Fellow, Iran.

Recent visitors at the National Bureau of Standards were **Frank Hudson**, metallurgist, Mond Nickel Company, London; **E. Nanai**, chief, Scientific Division, Nissan Chemical Industries, Ltd., Tokyo; **A. Sawada**, assistant works manager, Fuso Metal Industries, Ltd., Nagoya, Japan; **F. D. Richardson**, head, Nuffield Research Group in Extraction Metallurgy, Royal School of Mines, London; and **Olof E. H. Rydbeck**, professor and director of research, Electronics Laboratory, Chalmers University of Technology, Gothenburg, Sweden.

Grants and Awards

Awards totaling \$1,441,721 for scientific research and professional education in poliomyelitis have been approved by the **National Foundation for Infantile Paralysis**. The awards, effective July 1, will support 34 individual projects for research aimed at preventing the disease and improving treatment methods, as well as for the training of doctors, scientists, nurses, and other essential professional persons. These projects are in three categories—11 for virus research, 9 for treatment of after-effects of the disease, and 14 for professional education.

Awards for virus research to prevent or control the disease were made to: Yale University, **John R. Paul**, \$151,750 and \$90,638; Johns Hopkins University, **Kenneth F. Macey**, \$94,030; University of Pittsburgh, **Wil-**

Hammond Mc.D. Hammon, \$31,050; University of Michigan, *Thomas Francis*, \$81,500; New York University-Bellevue Medical Center, New York City, *David P. Earle, Jr.*, \$36,630; University of Minnesota, *Raymond N. Bieter*, \$14,000; George Washington University, *Paul K. Smith*, \$7,120; University of Minnesota, *Jerome T. Syvertson*, \$15,812; University of Washington, *Charles A. Evans*, \$26,930; University of Oregon, *Arthur W. Frisch*, \$5,275; University of California, Berkeley, *Wendell M. Stanley*, \$41,580.

Awards for the improvement of treatment methods went to: Cornell University Medical College, New York City, *David P. Barr*, \$4,700; University of California, San Francisco, *Robert B. Aird*, \$4,900; University of Minnesota, *A. B. Baker*, \$13,125; Northwestern University, *Lewis J. Pollock*, \$8,000; The Children's Medical Center, Boston, *William T. Green*, \$6,900; University of California, San Francisco, *LeRoy C. Abbott*, \$9,100; New York University, *Harold K. Work*, \$23,500; Syracuse University, *Otto H. Müller*, \$11,000; University of Minnesota, *Allan Hemingway*, \$4,404.

Educational awards were made to: Meharry Medical College, Nashville, *Murray C. Brown*, \$70,800; Washington University School of Medicine, St. Louis, *Robert A. Moore*, \$13,400; Northwestern University Medical School, *Stafford L. Osborne*, \$9,234; University of Southern California, Los Angeles, *Margaret S. Rood*, \$8,300; American Physical Therapy Association, New York City, *Mildred Elson*, \$53,762; National Organization for Public Health Nursing, New York City, *Anna Fillmore*, \$79,281. The balance of the 14 educational awards will be administered by the foundation to provide scholarships and fellowships for graduate training in professional fields.

Four awards for discoveries in carbohydrate chemistry and the functions of sugar have been made by the **National Science Fund of the National Academy of Sciences** administering a fund established by the Sugar Research Foundation. Claude S. Hudson, director of the Division of Chemistry, at the Na-

tional Institutes of Health, received \$10,000 for his research in the rarer sugars. Two awards of \$5,000 each were made jointly to Carl and Gerty Cori, of Washington University, St. Louis, for their work on carbohydrate metabolism in the animal body, and to Melvin Calvin and Andrew A. Benson, of the University of California at Berkeley, for study of photosynthesis. An award of \$5,000 was made to Maurice Stacey, of the University of Birmingham, England, for investigations of polysaccharides, particularly one that shows promise as a blood plasma substitute.

Twelve new grants for research in the field of nutrition have been announced by the **National Vitamin Foundation**. The foundation makes grants-in-aid of research semiannually. The new grants, which became effective on July 1, and total \$78,150, are: *A. E. Azelrod*, Western Pennsylvania Hospital, Pittsburgh, \$4,500 for the study of the relationship between nutritional factors and antibody production; *T. J. Cunha*, University of Florida College of Agriculture, \$2,500 for the study of the need of animal protein factor, B₁₂, B₁₂, and related factors for growth and the effect on food utilization, using pigs as the experimental animal; *E. W. Heinle*, Western Reserve University School of Medicine, \$6,000 for study of the hemopoietic role of vitamins on the B group in normal and pathologic conditions in animals and man; *L. Emmett Holt, Jr.*, New York University College of Medicine, \$5,000 for study of the thiamine requirement of the breast-fed infant; *M. K. Horwitt*, Elgin State Hospital, \$10,750 for studies of the B complex vitamins in human nutrition, with particular reference to niacin; *W. A. Krehl*, Yale University, \$1,600 for study of the relation of vitamins and hormones to amino acid metabolism; *Karl E. Mason*, University of Rochester School of Medicine and Dentistry, \$4,800 for a two-year study of the distribution of tocopherols in human tissues and organs during fetal life and early infancy and the implications to dysfunctions of pregnancy and early childhood; *Elaine P. Raitt*, New York University College of

Medicine, \$3,500 for study of vitamin-hormone relationships, using the rat as the experimental animal; *Nevin S. Scrimshaw*, Institute of Nutrition of Central American and Panama, \$8,000 for study of the effectiveness of vegetable protein supplemented by vitamin B₁₂ in the growth and development of undernourished children; *Thomas P. Singer*, Western Reserve University School of Medicine, \$5,000 for study of the mechanism of action of vitamin analogues on isolated enzymes; *Winslow T. Tompkins*, Pennsylvania Hospital, Philadelphia, \$22,500 for a three-year study of the significance of nutrition and nutritional deficiencies in human pregnancy; *Theodore F. Zucker*, Columbia University, \$4,000 for study of the effects of pantothenic acid on acetylation rates in man, with special reference to a possible role in duodenal ulcer.

Fellowships

A fellowship program for training of industrial physicians will be inaugurated this fall by the Atomic Energy Commission if qualified candidates are obtainable. Fellows will be sent to the University of Rochester Medical School and the University of Pittsburgh School of Public Health for the first year, at a stipend of \$3,600. The second year will consist of on-the-job training at a salary of \$5,000. Application forms are obtainable from the AEC Industrial Medicine Fellowship Committee, Atomic Energy Commission, Washington 25, D. C.

The **Arthritis and Rheumatism Foundation** is offering fellowships for research in the basic sciences related to the study of arthritis. The fellowships will be selected by the foundation's Medical and Scientific Committee. The one-year fellowships will carry a stipend of from \$4,000 to \$6,000, depending upon the needs and ability of the worker. Applications should be sent to the Arthritis and Rheumatism Foundation, 535 Fifth Avenue, New York City. Applications received by September 15 of this year will be acted upon at that time and notification of fellowships made immediately. All

applications must be received by January 1, 1951.

Summer Programs

The Second Berkeley Symposium on Mathematical Statistics and Probability will be held July 31-August 12 in the Statistical Laboratory, Mathematics Department, University of California. Cooperating with the university in sponsoring the symposium are the Biometrics Section of the American Statistical Association; the Biometric Society, Western North American Region; the Econometric Society; the Institute of Mathematical Statistics; the Institute of Transportation and Traffic Engineering; and the Office of Naval Research.

Reservations should be made as early as possible with Michel Loève, Acting Director, Department of Mathematics, Statistical Laboratory, University of California, Berkeley.

A summer session in post graduate optometric education will be held August 7-September 1 at the Los Angeles College of Optometry. The program is organized so that each week will constitute a complete unit of study. Although the courses are intended for those who will be in full-time residence, single courses may be taken by special permission. The fee for full-time attendance for each week is \$25. Further information and application blanks may be obtained from Dr. Monroe J. Hirsch, Director, Summer Session, Los Angeles College of Optometry, 950 West Jefferson Boulevard, Los Angeles 7.

Industrial Laboratories

Food Research Laboratories, Inc. has appointed Kenneth Morgareidge as chief chemist. Dr. Morgareidge was formerly director of the Biological Laboratories of the Nopco Chemical Company of New Jersey. He is best known for his work in the assay, stabilization, and control of vitamins, and the commercial production of vitamin concentrates.

Gustav Egloff, director of research, Universal Oil Products Company, Chicago, will address the Second Oil Shale and Cannel Coal

Conference (July 3-7) in Glasgow; the Fourth World Power Conference (July 10-15) in London; the Société de Chimie Industrielle, Association Française des Techniciens du Pétrole, l'Institut Française du Pétrole (July 18), Paris; the Associazione Italiana di Chimica (July 21), Milan; and Unione Petrolifera (July 25), Rome.

Nathan T. Melamed, research chemist, has joined the staff of Horizons Incorporated, of Princeton, New Jersey. Dr. Melamed did research on luminescent materials in the Brooklyn Polytechnic Institute Department of Chemistry from 1946 to 1949.

Frederic Fenger retired July 1 from Armour and Company Laboratories, after 44 years of chemical research in endocrinology. Dr. Fenger carried on early research with the thyroid and pituitary hormones, and contributed to the study of pepsin by producing it isoelectrically in pure form.

Meetings and Elections

The biological hazards of atomic energy will be the subject of a two-day conference at the Royal Institution in London, October 20-21. The conference is sponsored by the British Institute of Biology and the Atomic Scientists' Association, and supported by the British Association for the Advancement of Science and the British Institute of Radiology. Further information may be obtained from Prof. Alex Haddow, Chester Beatty Research Institute, Royal Cancer Hospital, Fulham Road, London, S.W. 3, England.

A conference on research in Alaska will be held in Washington, D. C., November 9-11, under the auspices of the National Research Council. U. S. and Canadian scientists in agriculture and forestry, botany, public health and medicine, zoology, fisheries, physiology, geology, geography, geophysics, meteorology, oceanography, and anthropology will discuss existing facilities for carrying out basic research and the establishment of scientific information centers in the U. S. and Alaska. This conference is the first coordi-

nated attempt to bring together information on various aspects of Alaskan research, to stimulate wider interest in such work, and to improve scientific cooperation among organizations and individuals engaged in it. Further information may be obtained from the Steering Committee, Alaskan Science Conference, 2101 Constitution Avenue, Washington 25, D. C.

The American Institute of Nutrition elected the following officers at its annual meeting in Atlantic City, April 17-21: president, Wendell H. Griffith, University of Texas; vice president, Clive M. McCay, Cornell University; treasurer, N. B. Guernant, Pennsylvania State College; councilor, Walter C. Russell, Rutgers University. Joseph H. Roe, George Washington University, continues in office as secretary.

The American Malacological Union elected the following officers at its annual meeting June 14-16: president, Joseph P. E. Morrison, Smithsonian Institution, Washington, D. C.; vice president, Jeanne S. Schwengel, Greenwich, Connecticut; second vice president, Leo G. Hertlein, California Academy of Sciences, San Francisco; treasurer, Harold R. Robertson, Buffalo, New York; and secretary, Mrs. Harold R. Robertson, Buffalo Museum of Science.

The Illinois State Academy of Science elected the following officers for the ensuing year: president, Percival Robertson, The Principia, Elmhurst; first vice president, James W. Neckers, Southern Illinois University; second vice president, Wayne W. Wantland, Illinois Wesleyan University; secretary, Leland Shanor, University of Illinois; and treasurer, Wilbur W. Grimm, Bradley University.

Deaths

Norman Ritner Beers died June 27 of a heart ailment at the Fort Hamilton Veterans Hospital, Brooklyn, New York. He was 38 years old. Dr. Beers had resigned as editor of *Nucleonics* in May because of his illness. He was formerly in charge

of the Meteorology Group at Brookhaven National Laboratory, Upton, Long Island.

Erik G. Hakansson, former commander of the Naval Medical Research Institute, Bethesda, Maryland, died June 19 at Chelsea, Massachusetts, after a brief illness. He was 63. Dr. Hakansson also served as chief of the Research Division of the Navy's Bureau of Medicine and Surgery until his retirement in 1948.

Harald M. Westergaard, 61, former dean of the Harvard Graduate School of Engineering and Gordon McKay Professor of Civil Engineering, died June 22 in Cambridge, Massachusetts. Dr. Westergaard was an authority on elasticity and was a member of a special mission to study the aftereffects of atomic explosions on structures in Japan.

The U. S. Army Hepatic and Metabolic Center has been transferred to Walter Reed General Hospital, Washington, D. C. The center, under the direction of Victor M. Sborov, carries on the Army's fight against infectious hepatitis, a disease that affected about 55,000 military men and women during World War II. Dr. Sborov is instructor in medicine at the University of Pennsylvania and research associate in medicine, Jefferson Medical College, Philadelphia. The center was established in 1948 and is closely associated with the Army's Commission on Liver Diseases. Cecil J. Watson, professor of medicine at the University of Minnesota, is chairman of the commission.

Research in plants and soils with radioactive isotopes will be carried on in a specially designed greenhouse at Plant Industry Station, Beltsville, Maryland. Studies using radioactive phosphorus, conducted by the U. S. Department of Agriculture in cooperation with the Atomic Energy Commission and Maryland agricultural experiment stations, have been in progress for the past three years. Work in the new \$250,000 greenhouse will include investigations with calcium, zinc, sulfur, and other radioactive elements.

A 50-Mev betatron, designed and constructed by General Electric, has been installed in the National Bureau of Standards' new betatron laboratory, extending the bureau's high energy research into the region from 2 to 50 Mev. For work at even higher energies, a 180-Mev synchrotron, now being completed by General Electric, will be installed at the bureau next year.

The Registry of Rare Chemicals, 35 West 33rd Street, Chicago, lists the following wanted chemicals: phosphoryl fluoride; ferric bromide, anhydrous; ferrous bromide, anhydrous; zinc carbonyl; ammonium cyanide; *p*-aminoethylbenzene; γ -aminobutyric acid; 2-phenylcyclohexanone; 3-methylpentanol-3; acetyl cyanide; 5-methylisoquinoline; acetyl phosphate; perfluorodipic acid; *n*-decyl mercaptan; *n*-hexadecyl mercaptan; methyl vinylsulfonate; methyl isopropenylsulfonate; perillie acid; scymmol; and coniferyl alcohol.

Recently Received—

Scientific Institutions and Scientists in Latin America: Argentina (Vol. 1), Uruguay (Vol. 1), Puerto Rico and Colombia (Vol. 1). Unesco publ. Centro de Cooperación Científica para América Latina, Montevideo.

First List of Scientific Papers Published in Latin America: 1948. Unesco publ. Centro de Cooperación Científica para América Latina, Montevideo, Uruguay.

Problems in the Collection and Comparability of International Statistics: Papers Presented at the Round Table on International Statistics. 1948 Conference of the Milbank Memorial Fund. Milbank Memorial Fund, 40 Wall St., NYC. 50 cents.

Contact Transients in Simple Electrical Circuits (PB 99613) National Bureau of Standards publication. Office of Technical Services, U. S. Department of Commerce, Washington 25, D. C. 50 cents.

Tables of Binomial Probability Distribution. National Bureau of Standards Circular C483. U. S. GPO, Washington 25, D. C. \$2.50.

Testing by the National Bureau of Standards. Circular 483. U. S. GPO, Washington 25, D. C. 25 cents.

National Conference on Cardiovascular Diseases, Summary Report. American Heart Assn., 1775 Broadway, New York City. \$90 per 500, \$20 per 100, \$11 per 50, 25 cents each up to 50.

International Statistics Conferences, Proceedings. September 16-18, 1947. Vols. II and IV. Joint Arrangements Committee, International Statistical Conferences of 1947, CARE Monumental Printing Company, Baltimore 11, Md.

Meetings Abroad

International Colloquium on Rheology in Biology, July 26-28, Lund, Sweden.

International Union Against Venereal Diseases, July 29-August 2, Zurich, Switzerland.

Seventh International Congress of Surgery, August 1-5, Buenos Aires.

Sixth International Congress on the History of Science, August 14-21, Amsterdam.

18th International Physiological Congress, August 15-18, Copenhagen.

Fifth International Congress of Microbiology, August 17-24, Rio de Janeiro.

Biennial Congress of the International Society of Hematology, August 21-26, University of Cambridge, England.

International Society of Sugar Cane Technologists, August 25-September 16, Queensland, Australia.

International Industrial St. Erick Fair, August 26-September 10, Stockholm, Sweden.

Symposium on Physiological Mechanism of Lactation, August 28-September 2, Strasbourg, France.

First World Conference on Alcohol and Traffic Safety, August 30-September 1, Stockholm.

British Association for the Advancement of Science, August 30-September 6, Birmingham, England.

Published this year for chemistry classes

Bray, Latimer and Powell:

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This form of the third edition substitutes rapid, equipment-saving semi-micro techniques for macro techniques throughout. Based on the belief that laboratory work should be the core of the course in general chemistry and qualitative analysis, it contains the equivalent of eight pages of line illustrations, depicting proper methods of manipulating laboratory materials. *Published in May. \$3.00*

Conant and Blatt:

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Subtitled A BRIEF COURSE FOR STUDENTS CONCERNED WITH BIOLOGY, MEDICINE, AGRICULTURE AND INDUSTRY, this text was written specifically for the groups indicated. Emphasis is on the biochemical and industrial aspects of organic chemistry—those reactions which are either essential to life or provide important industrial products. Treatment of biochemical processes, of heterocyclic compounds, and of chemotherapy is outstanding. *Published in March. \$4.00*

Louis J. Curtman:

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Dietrich and Kelsey:

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Sisler and Stewart:

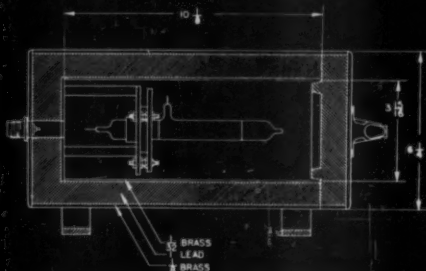
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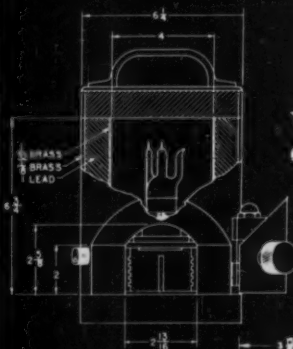
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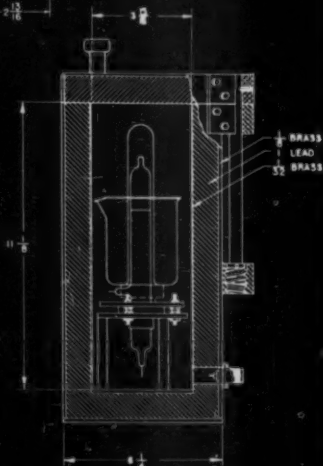


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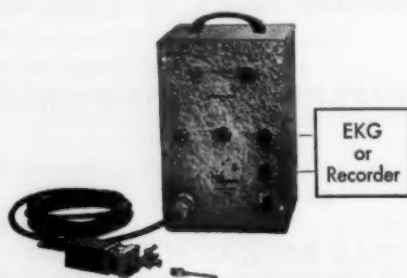
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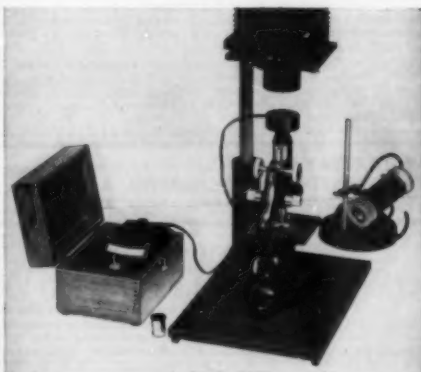
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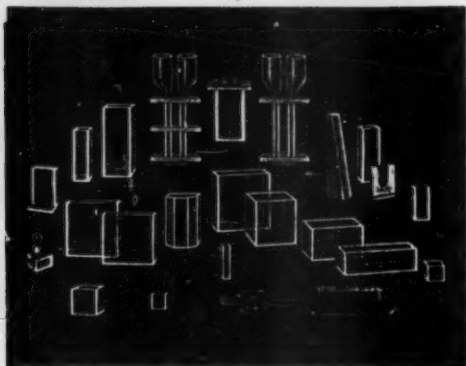
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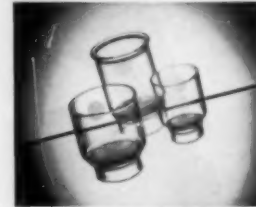
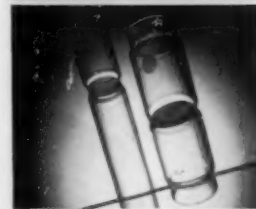


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*For technical information regarding flow rates and pore sizes of Fritted Glassware, write Corning Glass Works for Bulletin B-80.



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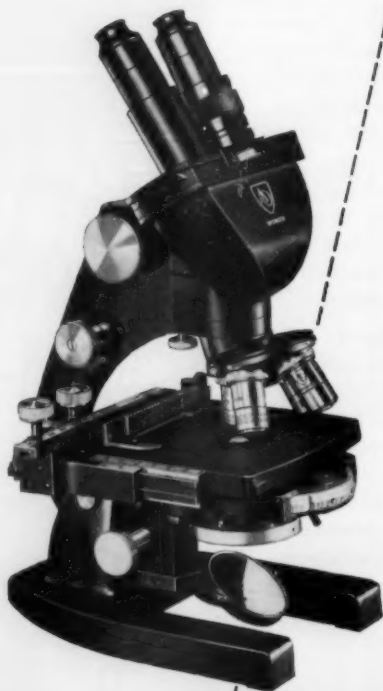
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